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SECOND REPORT ON A LAMARCKIAN EXPERIMENT

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SECTION I.

(1) *The earlier experiments and their modifications.*

IN 1927 in the April number of this *Journal* I made a first report on the progress of an experiment for the testing of the Lamarckian hypothesis, describing the methods used and the results obtained in the period 1920 to June 1926. The experiment has been continued and has yielded further results that seem to justify a second report at this time. In the first period two halves of a stock of pure-bred white rats were trained generation by generation in two different tasks. One task consisted in learning to escape from a water-maze; the other in learning to escape from a tank of water by the less brightly illuminated of two gangways. Both procedures seemed to give some evidence of increasing facility in mastering the tasks in successive generations. But the maze-procedure seemed in several respects less satisfactory than the tank-procedure[1]; it has, therefore, been abandoned, and the present report deals only with the continuation of the tank-experiment from 1926 to 1929.

[1] Especially in that success in the task seemed to be favoured by general vigour, a factor liable to wide variations that cannot be satisfactorily controlled.

During the first period 13 generations of rats were trained in the tank. During the second period 10 more generations (14–23) have been trained; and the increase of facility shown by the later generations is so great as to leave no room for doubt of the objective reality of the superior facility implied by the figures representing the performances of the rats of the trained stock. This peculiar facility implies a constitutional peculiarity induced in the tank-trained stock. (Henceforth the rats of this stock are called Tank-Rats or T.R. and this constitutional peculiarity is referred to as X.) Two questions of great interest arise.

The first and more important question is: Is it possible that X has been acquired by the tank stock through some selective process?

The second question is: Can we define more nearly this factor X?

I shall first report briefly the progress of the experiment and then consider these two questions and the experimental evidence bearing upon them.

The tank-procedure described in the former report has been modified in two respects, neither of which involves any essential modification of the task imposed on the animals. During the first period, owing to lack of time, I did not carry through to completion the training of all the rats of each batch. During the second period, having more time for research work, I have carried the training of every rat through to completion, *i.e.* to the point at which the rat ceases to make any errors and has completely mastered his task. This modification brings two advantages. First, it enables me to state the degree of facility displayed by any rat or group of rats in a simple and satisfactory way, namely, to state the number of errors made by the individual rat in the complete course of training, or the average number of errors made by the rats of any group. Such number of errors seems to be a fairly exact indication of the position of the rat in the scale of facility or of the degree of X possessed by the individual. The average number of errors of the group is a still more satisfactory indication of the constitution of the group in respect of X, especially when supplemented by figures indicating the range of the number of errors within the group.

Secondly, it now appears to me not improbable that the training process preceding the final or critical stage of learning (through which each rat finally graduates to become a finished performer) is of little or no effect; and that the Lamarckian transmission that seems to occur is a function of the achievement of the critical stage of learning.

The second modification of the procedure consists merely in a simplification of the task of the rat in a way that avoids considerable waste of time; namely, a curved sheet of metal is inserted in the tank

in such a way as to close the two blind alleys of the tank described in the former report. The tank of the second period has, then, the form indicated in Fig. 1 (cp. Fig. 1 of the former report). The curved line indicates the sheet of metal which shuts off the blind alleys of the further end and confines the rats to the part of the tank nearer the platform *P*.

As in the later part of the first period, the alternating procedure was applied throughout this second period. The rat to be trained is repeatedly placed in the water at *O*. He can escape from the water to the platform *P* only by climbing one of the two gangways *A* and *B*. On each occasion one of the gangways is brightly illuminated from behind and is so wired that the rat, as he emerges from the water on to the gangway, receives a secondary interrupted current through his feet[1]. After each immersion both the shocking and the illuminating current are switched over to the other gangway. Each rat is immersed at *O* 6 times on each day of training (a day occasionally being omitted) until he makes 12 successive escapes without error[2]. The total number of occasions on which the rat has taken the illuminated gangway and received the shock is accepted as the inverse measure of his facility in mastering the task or solving the problem. It is to be noted that each taking of the illuminated gangway is reckoned as one error. The number of errors made by any rat is then the same as the number of shocks required in order to induce unfailing avoidance of the bright gangway.

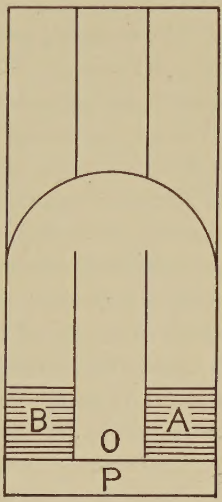

Fig. 1.

(2) *Details of the later experiments.*

Up to June, 1926, rats of 13 generations had been trained in the tank (cp. first report). During the academic year, 1926–7, I was travelling, and my assistant, Mr E. C. Heck, who had already co-operated in the experiment, trained in the tank the 14th, 15th and 16th generations.

Of the 16th generation only 5 rats were trained, and these 5 begat four litters from which 11 rats were taken at random and trained as

[1] Cp. footnote, p. 204.

[2] Experiment shows that any rat that has attained this level of proficiency will very seldom make another error. Even if tested after an interval of 2 or 3 months, an error is seldom made; and a single error usually then suffices to restore complete proficiency in avoidance of the shock.

the 17th generation. Of the 18th generation 22 rats were trained; of the 19th generation 15 rats. Of the 20th generation the training was incomplete owing to unavoidable interruption of work. Of the 21st generation 28 rats were trained; of the 22nd generation 16 rats; and of the 23rd generation 26 rats. In each generation the young for training were chosen at random from all the litters born by the trained animals of the preceding generation, approximately in equal numbers from each litter. From the 14th generation onward, among the rats that entered upon training in each generation, were a small number (not more than 3 of any generation) whose training was not completed, owing in nearly every case to the rat receiving a shock which incapacitated him[1].

The average number of errors of the group of rats of each of these generations is stated in Table I. Although of the 23 rats of the 13th generation 6 rats were not completely trained, it is possible to form a fairly trustworthy estimate of the average number of errors of the whole batch of 23, and I have, therefore, added this to the table[2].

Since the range of facility shown by the rats of each generation is wide, I have added to the table the figures expressing the number of errors made by the best (or most quickly learning) rat and the worst (or most slowly learning) rat of each group.

Table I contains also the corresponding figures for several batches of control rats. Of these the group W.C. 1926 were the progeny of untrained parents newly obtained from the Wistar Institute. They were of the same stock as my original stock and had been (like my own stock) inbred and subjected only to the slight selection involved in rejecting the occasional runt or obviously weakly member of a litter. The training of this group was not carried quite to completion, *i.e.* a few of the 17 rats remained incompletely trained; hence the figure 170 given as representing the average error of this group is approximate only, but it is very improbable that it varies from the true figure by more than five digits more or less.

The group W.C.$_1$ 1927 were 10 rats newly obtained from the Wistar

[1] This was either a shock through the head which in some cases was immediately fatal, or one which, in some way that remains obscure, paralyzed the hind quarters. Mortality from other causes has been very slight.

[2] The average number of errors of the whole group of 23 rats up to the 30th day of training was 68. At this point 6 rats still made errors. Of the 17th, 18th and 19th generations 5 rats made errors after the 30th day. I have added the number of errors made by these 5 rats to the number made by the 13th generation up to the 30th day. This brings the average error for that generation up to 75. It is highly probable that, if the training had been completed, the average number of errors of the 13th generation would have been rather greater than 75.

Institute and of the same stock as my original stock of 1920 and the controls of 1926.

The group W.C.$_2$ were selected at random from all the litters produced by the group W.C.$_1$ 1927.

The group W.C.$_3$ were selected in the same strictly random fashion from all litters produced by group W.C.$_2$.

These three groups (W.C.$_1$, W.C.$_2$ and W.C.$_3$) were trained with the intention of repeating the early stages of the training process with special attention given to avoidance of any favourable selection. When the Lamarckian effect of training seemed to be clearly shown by W.C.$_2$ and W.C.$_3$, I decided to adopt for later generations a more rigid and sure method of ruling out the possibility of any unwitting favourable selection. Of the 37 rats of group W.C.$_3$, 5 individuals (2 males and 3 females) were very decidedly inferior to all the rest in respect of rate of learning. Whereas the 'average error' of the group was 144 and the errors of the best rat of the group were only 45, the errors made by these 5 rats ranged from 204 to 229, their average being 215. That is to

Table I.

Tank rats			No. of errors of best rat		
	Rats	Average no. of errors per rat*	Errors	Rats	No. of errors of worst rat
13th gen.	23	68 + (probably 75 +)	30	1	?
14th ,,	10	80	42	1	102
15th ,,	10	70	39	1	96
16th ,,	5	73	39	1	88
17th ,,	11	46	9	1	147
18th ,,	22	62	15	1	142
19th ,,	15	47	12	1	100
21st ,,	34	37	9	3	74
22nd ,,	16	36	6	3	89
23rd ,,	26	25	3	2	71
Control rats (unselected):					
W.C. 1926	17	170†	132	2	?
W.C.$_1$ 1927	10	164	90	1	299
W.C.$_2$	22	149	66	3	234
W.C.$_3$	37	144	45	2	229
W.C.$_4$	10	114	42	1	170
Control rats (adversely selected):					
W.C.$_3$	5	215	204	2	229
W.C.$_4$ (W.H.)	18	166	75	2	241
W.C.$_5$ (W.H.)	11	134	81	3	196

* All figures of this column state nearest whole number.

† The training of this group (W.C. 1926) was carried only to the 45th day, when 10 rats still made errors. The average number of errors actually made up to the 45th day was 137. The figure 170 is a conservative estimate reached by adding to the total of errors actually made up to the 45th day, the number of errors made by the 10 rats of the group W.C. 1927 after their 45th day of training.

say, these 5 rats were, in respect of facility in this task, a nearly homogeneous group of very low facility; they were decidedly slower to learn than the average rat of untrained stock. I took these 5 rats as the ancestors of the later generations to be trained. I trained all the three litters produced by these 3 slow females crossed with the 2 equally slow males. These three litters are the 18 rats of group W.C.$_4$ (adversely selected).

These 18 rats were trained in three batches each of 6 rats. From each of the three batches the 3 best performers were set apart as Stock W.C.$_5$ (B.H.) (*i.e.* Wistar controls—better half). The worse half of each batch were then interbred and from their several litters 12 rats were taken at random to form group W.C.$_5$ (W.H.). The group W.C.$_5$ (W.H.) thus is the product of a severely adverse selection exercised on the two preceding generations, W.C.$_3$ and W.C.$_4$. It is my intention to continue for some further generations the process of breeding from the worse half of each group trained and to train from among the progeny of this worse half a group selected in strictly random fashion. I can think of no surer method of excluding favourable selection.

(3) *Analysis of results of the later experiments.*

The main features of interest of Table I are the following:

(1) Although, unfortunately, I have no record of the average of T.R. 1st generation, it is a fair presumption that its 'average error' would not have been widely different from that of the groups W.C. 1926 and W.C.$_1$, both of which are rats of the same Wistar Institute stock whose ancestors have undergone no training. The average error of T.R. 1st generation could hardly have been less than 150; in all probability it would have been about 165 or a little higher. The generations 13 to 23 show very marked increase of facility as compared with the controls of untrained parentage (W.C. 1926 and W.C.$_1$) and with the presumptive facility of T.R. 1st generation; the facility reaching a maximum in T.R. 23rd generation, whose 'average error' is only 25.

(2) This superior facility of the generations 13 to 23 is revealed not only by the diminution of the average error (from the neighbourhood of 150 to 25 in generation 23) but also by the facts indicated in the last two columns. In the 23rd generation 3 rats have such facility in learning that they make only three errors apiece, *i.e.* have learnt to avoid the bright gangway entirely after only three shocks from it; whereas the rat of greatest facility of the untrained control stock makes 90 errors, or requires 90 repetitions of the shock, before he learns to avoid

entirely the bright gangway. Again, all the slowest rats of the generations 13 to 23 are very much superior to the slowest of the untrained stock. And the slowest rat of T.R. generation 23 is decidedly superior (71 errors) to the best rat of the untrained stock (90 errors). *There is thus no overlap* between T.R. generation 23 and the untrained stock; every member of T.R. generation 23 is superior to the best of the untrained stock, in spite of the very wide range of the facility in all groups. The same is true of T.R. generations 21 and 22.

(3) There is fairly steady improvement of the T.R.'s from generation 13 to generation 23, shown in all three columns of the table.

(4) There is similar improvement shown in all three columns from W.C.$_1$ to W.C.$_4$ of the unselected controls.

(5) The very severely adverse selection applied in producing the group W.C.$_4$ throws back this group so that it is inferior in facility to W.C.$_1$ (the 5 parents of group W.C.$_4$ gave 'average error' of 215).

(6) In spite of the further adverse selection applied to produce group W.C.$_5$, this group shows advance on its parents and is superior in all three columns not only to the untrained stock (W.C. 1926 and W.C.$_1$, 1927) but also to W.C.$_2$, and is superior in columns 1 and 3 to W.C.$_3$. This seems to imply that the Lamarckian transmission of acquired facility overcomes the opposed influence of strongly adverse selection. This would seem to be a most crucial experiment; and it will, I hope, be continued through a number of generations. The large advance of group W.C.$_4$ upon their parents may be regarded as, in part at least, a phenomenon of regression towards the mean. But this explanation will not hold for the further advance of group W.C.$_5$, since this advance carries the group beyond the mean of the grandparental performance.

SECTION II.

(4) *Objections to the application of the Lamarckian hypothesis.*

In this section I propose to discuss certain possibilities of explaining away the results shown in Table I as having been produced in some other way than by Lamarckian transmission, some of which have been suggested by critics of my former report, to whom I return thanks.

(1) Can it be that the superiority of the trained generations is due to some direct influence of the trained parents upon the offspring either during intrauterine life or in the period between birth and the end of training?

It should here be said that the training of each batch of rats was

begun in nearly all cases about the end of the 4th week and that the mother was allowed to remain with the young up to about the end of the 5th or 6th week. The superiority of the young T.R.'s to the controls might then be due (*a*) to the transmission to the embryo of some hormone (*e.g.* a fear-exciting hormone), (*b*) to actual instruction or warning given by the mother, (*c*) to timidity or other emotional state induced in the mother by the training-process and imparted in some degree to her progeny by sympathetic induction, (*d*) to some 'telepathic' impression from mother to young.

(2) Can it be that rats of the trained stock are unduly favoured by me in the course of training; that I, being naturally interested in, or desirous of finding, an increase of facility in the successive generations, have, in various subtle ways not easily to be defined, favoured the trained stock?[1]

(3) Can it be that all the differences of rate of learning throughout the very wide range (expressed by 3 errors of the best T.R.'s and the 299 errors of the worst W.C.'s) are not expressions of genetic or constitutional differences, but are purely or in the main due to 'accidental' influences exerted on the rats before and during training? This last, I suppose, is the view to which the extreme behaviourist would incline.

(4) Can it be that, in spite of my efforts to avoid it, selection favourable to the increase of X has been exercised to an extent capable of producing the superior facility of the later generations?

There are, I submit, ample grounds for ruling out all these vague possibilities. The steady increase of the induced facility throughout the generations answers these four questions in the negative.

And this answer to all these questions is strongly supported by the following experiment.

(5) *The cross-breeding experiment.*

The 3 very slow mothers of W.C.$_3$ (each of which made about 215 errors) were mated with the 2 slowest males of the same group (each of which made about 210 errors). The 18 progeny resulting from this mating gave an 'average error' of 166. The same 3 mothers were then mated with 3 males of the T.R. 21st generation, each of which had made during training approximately 40 errors.

[1] In this connection the vague possibility of some 'telepathic' influence from me to the rat must be regarded seriously in view of Bechterew's claim to have demonstrated telepathy between man and dog, and Dr Rhine's seemingly successful telepathic experiments with a horse.

From these crosses (between the females of slightly trained stock and very low facility and males of much trained ancestry and high facility) there resulted three litters of 16 rats. These 16 cross-breds gave an 'average error' of 62 (and the average errors of the three litters were nearly identical, namely 65, 65, 56), and a range of errors from 8 for the best rat to 126 for the worst.

After training, these 16 cross-breds were interbred, giving a large number of litters of F_2 generation. From these many F_2 young, 12 were selected at random and trained. They gave 'average error' of 64 and ranged from 39–114. That is to say, the F_2 generation showed almost exactly the same facility as the F_1 generation; and both showed very strongly the influence of the fathers of highly trained stock and high facility.

These cross-breeding results leave little room for doubt that the degree of facility displayed by an animal, and still more the average facility of a group, is a function in the main of some factor or factors X in the genetic constitution of the animals, and that this factor is transmitted by both the male and the female. In these cases the father came into no kind of relation with his offspring other than through his sperm cells. The experiment, then, goes far to rule out all explanation through parental influences other than through the germ cells.

Further evidence bearing on question (3), and tending to show that the facility displayed by any group is in the main a function of its genetic constitution, is the following. We have seen that the 5 slowest rats of group W.C.$_3$ (av. er. = 215) produced slow offspring (av. er. = 166). Of this same group W.C.$_3$, 4 rats, all of which gave about 130 errors, were interbred and their progeny of 10 rats gave 'average error' of 114.

Again, while the worse half of the group W.C.$_4$ produced progeny whose 'average error' was 134; the better half of the same group produced progeny whose 'average error' was 70. These are only a few special instances illustrating the general rule which holds throughout, namely, that, in respect of this particular facility, the offspring show pretty close resemblance to their parents, a resemblance which is the closer the larger the groups of parents and offspring compared.

I turn now to consider in more detail the most insidious possibility of error, namely, that suggested in question (2). There are, I submit sufficient grounds for a confident negative answer to this question.

First, the different rates of learning of the rats of different groups is not the only evidence of a constitutional difference between them. In the later T.R. generations it became more and more obvious that the

behaviour in the tank of the rats of trained stock distinctly differs from that of rats of untrained or only slightly trained stock. The difference can best be expressed by saying that the behaviour of the rats of highly-trained stock is markedly more tentative, hesitating, cautious, or exploratory than that of the other rats. Whereas the control rats in the early part of their training loiter very little, hesitate very little, and, having emerged into either lateral passage of the tank, rush, almost without exception, straight at and on to the gangway; the rats of the later T.R. generations are apt to loiter long near *O*; when they emerge from *O* they are apt to hesitate and oscillate between the two lateral passages; and, having entered one passage, they will frequently turn back and enter the other, in many cases repeating this turning back many times. Even when they approach closely to either the dim or the bright gangway they may turn back from it; and not infrequently they approach cautiously the bright gangway, touch it lightly with nose or paw, receive a slight shock and forthwith retreat. I have never observed such cautious or tentative behaviour in any rat of the control stock at an early stage of training. It is true that the rats of the control stock eventually, as they approach the critical point of their training, begin to exhibit something of this tentative or cautious behaviour, loitering, hesitating, oscillating and rarely turning back after having approached the bright gangway. It seems, then, that the rats of the later T.R. generations develop spontaneously (or in response to a very few shocks) a hesitancy and caution which is induced in the control rats only by a long course of training involving many repetitions of the shocks; or loosely, the rats of T.R. 23 are born in that condition which the control rats reach only after much training. So marked is this difference that either Dr Rhine or myself could generally distinguish pretty confidently rats of the two stocks merely on seeing them immersed in the tank for a few seconds at an early stage of training[1].

But perhaps the best negation to question (2) is afforded by the following facts. In training the generations W.C.$_2$, W.C.$_3$, W.C.$_4$ and W.C.$_5$, I was just as keen to see them make a good record as I was in training the later T.R. generations; and these generations of the two stocks were trained under exactly similar conditions, on the same days and in immediate succession, often in alternating batches. If, then, my desire to see a batch of rats make a good record could in any subtle

[1] During the academic year 1928–9 I have had the co-operation of Dr J. B. Rhine, a well trained biologist, a most careful worker. I have to thank Dr Rhine most heartily for his assistance and also the President of Duke University who made it possible.

way lead me to favour that batch, it should have worked equally favourably on the W.C.$_2$, W.C.$_3$, W.C.$_4$ and W.C.$_5$ generations, as on the later T.R. generations. But the difference between the 'average errors' of the two stocks shows that if, in spite of the best intention to be impartial in my handling, I do not altogether succeed in avoiding partiality, this is an influence of very minor importance which can neither account for nor disguise the very large differences of constitution between the two stocks.

In this connection I make special report on the intensity and duration of the shock delivered to each rat as it takes the bright gangway. For it is in this respect that the danger of partiality is most insidious.

It is obviously desirable that in these respects the shocks delivered to the rats should be as nearly as possible uniform. Complete standardization is impossible, since the intensity of the shock received must vary not only with the strength of the primary current, but also to some extent with the somewhat variable behaviour of the interruptor and with the kind of contact made by the rat. The principle I have followed is to adjust the secondary coil at such a distance from the primary that the rat's limb-muscles are tetanized if he makes good contact. If under these conditions a rat rushes up the gangway with great energy he may escape tetanization, though, as shown by his squeak and other details of behaviour, he does not fail to receive the shock. If the rat is tetanized, he is held during 3 seconds; when the current is broken and he runs up the gangway. Since, then, standardization of the intensity and duration of the shock is very imperfect, it is of some importance to know whether and to what degree different degrees of shock conduce differently to rapidity of learning.

Dr Rhine has therefore made for me the following experiment. He used shocks of three degrees of intensity, light (just sufficient to make the rat squeak as he passed up the gangway) medium (almost sufficient to tetanize the limbs, *i.e.* would tetanize if the rat made exceptionally good contact) and heavy (strong enough to tetanize in almost every instance); the tetanization being continued for 3 seconds.

Three litters of W.C.$_4$ generation (offspring of the unselected W.C.$_3$ group) were split into three groups of 6, 2 rats from each litter; and one group was trained under each strength of shock up to the 82nd day, when the experiment had to be discontinued. Under the light shock no rats learnt; under the medium shock 1 learnt on the 54th day, 1 on the 71st; the other 4 failed to learn. Under the heavy shock 1 learnt on the 52nd day, 1 on the 57th and 1 on the 59th day; the other 3 failed to learn.

He repeated this experiment with groups of rats of T.R. generation 23; making up each group of 4 rats by splitting litters between the groups. Of these the 4 rats under light shock learnt in 24, 26, 27 and 29 days. Those under medium shock learnt in 17, 19, 19 and 21 days. Those under heavy shock learnt in 8, 9, 10 and 14 days.

This experiment seems to show pretty definitely that the heavier the shock up to the point of tetanization the more favourable is it to rapid learning; though the difference between the effects of medium and heavy shock is not very marked.

Since all my shocks fell undoubtedly within the range of the medium to heavy shocks of Dr Rhine's experiment, it follows that there was not wide scope for unwitting partiality on my part in the administration of the shocks, no such partiality as could account for more than a small fraction of the differences of facility revealed in the training process.

But I have given the foregoing account of this experiment of Dr Rhine's with a second object in view; namely, the results of the experiment, conducted by this independent and very careful worker, bring out very clearly the large superiority of facility of the T.R. 23rd generation to the $W.C._4$ generation—the difference revealed in his experiment with shocks of three different strengths corresponds pretty well with the difference brought out by my training of groups of these two generations (namely, $W.C._4$ gives av. er. 114 while T.R. 23 gives av. er. 25). The difference found by Dr Rhine is in fact rather greater than that found by me in equivalent groups. The average error made by Dr Rhine's T.R. 23 group under heavy shock was 22—thus a little better than my finding. While of his $W.C._4$ group (though the training was not completed) the average error under heavy shock cannot have been less than 165 (thus rather worse than my finding).

In my view this confirmation by Dr Rhine (which is only one of several similar instances) of my findings leaves no room to doubt that the differences of facility revealed by my training procedure are real and objective facts and are not due, in any appreciable measure, to defects of my manipulations.

(6) *Selection and improvement.*

Lastly, a few words about the possibilities of selection having produced, or favoured the production of, this change of constitution implied by the greatly increased facility of the later generations.

My method of selection of T.R.'s for training in each generation is to make up batches of 6 rats by taking at random an equal number of

rats from each of several litters of approximately the same age; and to secure that all litters born of the parent generation (after training) are represented by about equal numbers. Thus if ten litters are born of the parent generation, I take at random (rejecting only the occasional obvious weakling) 2 rats from each litter and place them in the group to be trained. Of course in any particular generation this process might fortuitously result in a favourable selection. But it is just as likely to result in an unfavourable selection. And in the course of many generations, the chances against any large predominance of favourable selections must be very great.

It is unfortunate that, of the 16th generation T.R., 5 rats only were trained owing to the pressure on Mr Heck's time. So small a number gives scope for a markedly favourable fortuitous selection. But that the selection was not markedly favourable is shown by the relatively high 'average error' of the group (73) as well as by their range of error.

In this connection I insist again on the advance shown by the generations W.C.$_4$, W.C.$_5$, in spite of strongly adverse selection.

SECTION III.

(7) *The nature of the peculiarity induced by the training process.*

In order to establish the reality of Lamarckian transmission it is not necessary to be able to define exactly the nature of that which is transmitted. It suffices to show that the behaviour of successive generations is modified in the direction of the behaviour induced by training of the parents, or, as in the present experiment, to show that, in successive generations of the trained stock, facility in acquiring a certain mode of behaviour increases generation after generation. I know that in face of such facts as are presented in the foregoing pages some of the neo-Darwinians will resort to dialectics. They will assert that there is no essential difference between white and black since both are but shades of grey; that whiteness and blackness are but varying subjective interpretations of one phenomenon; that in fact all cats are grey. I incline to believe that the difference between white and black is real, and that the difference between modification of a stock by selection on the one hand and by transmission of acquired modifications on the other is equally real, and is not to be conjured away by any verbal jugglery, *e.g.* by pointing out that in reality no characters are transmitted but only potentialities of development or of reactions to environmental stimuli.

It would close the door to some of this verbal jugglery if we could define more nearly that X which seems to be transmitted as the innate basis of the superior facility of the trained stock. Dr Rhine is directing his experiments largely to this question, and I hope that in due course he will publish an article that will throw considerable light upon it. In the present article I will indicate only very briefly some possibilities and some light on them which we have already obtained. Reminding the reader that by X I mean that in the innate constitution of the rat under training which gives the T.R.'s of later generations marked superiority to the control rats and to T.R.'s of earlier generations in respect of facility in learning to escape from the tank by the dim rather than the bright gangway, I state and partially and provisionally answer certain questions without describing the experimental evidence on which the answers are founded. This evidence will, I hope, be set forth in a later article by Dr Rhine.

(1) Is X merely a lower degree of vitality or vigour which predisposes the T.R.'s to go slowly and tentatively about their task?

There is no evidence that the T.R. stock is less vigorous than the control stock. And several litters of control stock intentionally brought up to be less vigorous than normal litters (by leaving very large litters of 12 or more young to be suckled by the one mother) have shown no superiority in learning to the normal litters.

(2) Is X an increase of 'general intelligence,' of Spearman's G factor? This interpretation is suggested by the fact that the rats of the stock trained for some generations in the water-maze showed in the tank task a facility markedly greater than that of the control rats[1].

We have made experiments which seem to show clearly that this cannot be a main or important part of X.

(3) Is X an abnormally great general timidity? Such timidity might either be transmitted through the germ-cells or by chemical diffusion (of a hypothetical fear-hormone) from the blood of the mother.

The cross-breeding experiment (p. 208) seems to rule out the latter alternative. It is further negatived by the fact that a few W.C. mothers were trained up to the day before parturition, and their progeny showed no excess of facility over other litters of W.C.'s of the same generation. The former alternative is shown to be of no great importance, if any, by the fact that the T.R.'s show to those long familiar with them no signs of abnormal general timidity; whereas some of the very large litters of W.C.'s were not only lacking in vigour and undersized, but showed

[1] Cp. first report in this *Journal*.

so much general timidity that it obtruded itself on the notice of casual observers; yet showed no superior facility.

(4) Is X a specific fear of the bright gangway, or a special liability to fear in the particular circumstances of immersion in the tank? This seems to be equivalent to asking—Is X a form of 'conditioned-reflex' in the sense in which Pavlov uses that term?

The hesitant, cautious behaviour of the T.R.'s in the tank lends colour to this view. It appears that every rat, before it learns to avoid the bright gangway, must attain this stage of hesitant cautious behaviour in the tank. In this stage of hesitant behaviour it would seem that the rat is the seat of a real conflict of impulses; of the impulse to rush to the gangway so as to escape from the water and of the impulse to retreat from the gangway. Such hesitation, such conflict of opposed impulses, is the beginning of wisdom, is the necessary pre-condition of a discriminatory judgment. The behaviour of the rats in this stage of learning implies very clearly such conflict of impulses.

It is an interesting question—How does the repetition of the shock on the bright gangway eventually bring every rat to effect discrimination, avoidance of the bright and choosing of the dim gangway? A rat may suffer the shock many (say 200) times, and still rush headlong at both gangways, without the least sign of hesitation or effort to discriminate. Yet, after further repetitions, he begins to hesitate, to turn from one to the other, and then, after a shorter or longer period (usually extending through some 2 or 3 days of training) of such hesitation, achieves the discrimination. It seems natural to suppose that the successive shocks produce a cumulative effect, which, although insufficient at first to modify behaviour overtly, does so modify it in the direction of hesitation when the effect reaches a certain degree or level. Yet an alternative possibility suggests itself. Perhaps there is no such cumulative effect. Perhaps there is in the brain some barrier that must be broken down before the experience of the shock can lead to hesitation. If so, then the rat which hesitates after a few shocks may differ from the rat which begins to hesitate only after many shocks in that this barrier in the former is less tough (is perhaps of lower synaptic resistance) than in the latter. And it may be that the breaking down of the barrier requires not only the shock but also some favourable conjunction of conditions. The rat with the weaker barrier will then need for the breaking of it a less favourable conjunction (a constellation of fewer favouring conditions) than is required by the rat with the stronger barrier. If so, then the former rat runs a better chance of meeting with

the required conjunction at an early period of its training. Hence, on the average the weaker a rat's barrier, the earlier in the course of training will it begin to hesitate.

It would seem that any impulse of avoidance evoked by the sight of the bright gangway cannot, in the first period of training, contend successfully against the strong impulse to escape as rapidly as possible from the water by the visually presented route. But in the second period of training, the period of hesitation, the impulse to retreat from or to avoid the gangway enters into effective conflict with the impulse to rush up the gangway. But such conflict of impulses, though a necessary condition of success, is not enough. Something more is needed, namely, the effectuation of a discriminatory judgment. At first both gangways evoke the impulse of retreat, and the rat oscillates, looks first at one, then turns about and looks at the other, in some cases approaching nearly to each gangway again and again. Among the T.R.'s of later generations occur individuals which show so strong an aversion from both gangways that they refuse to ascend either; on some occasions of immersion they would drown if I did not take them from the water. That is to say, these individuals quickly enter the stage of effective conflict of impulses; but they are slow to achieve discrimination. The phase of hesitation seems to be terminated more or less abruptly by a definite act of discrimination, constituting a critical point in the process of learning. This is implied by the changed behaviour of the rat. It is not that in this phase he never enters the brightly lit passage or approaches the bright gangway; it is rather that, if he does so, he turns about and swims swiftly and directly to the other gangway and so out.

(5) Is it possible that X consists in part or wholly of a toleration for, or diminished aversion from, the water, such that immersion evokes a less strong impulse to escape from the water? Such diminution of aversion would permit the impulse of retreat from the gangway to enter more readily (at an earlier stage) into effective conflict with the impulse to escape.

Direct observation of the rats when first immersed reveals no such diminished aversion. The T.R.'s of later generations seem at first as urgent to escape from the water as the controls; though there may be a slight difference in this respect. It is only after receiving one or more shocks on the gangway that the hesitant behaviour clearly appears. The time spent in the water before escaping is then very much prolonged as compared with the time spent in the water before the hesitant phase sets in. Before the beginning of the hesitant phase the time spent in the

water on each immersion is little more in all cases than the minimum time required for escape; in the hesitant phase it is commonly prolonged to one or two minutes, and in many cases is still longer.

There would seem, then, to be two main factors making for success or rapidity of learning: first, the readiness with which the impulse to retreat from the bright gangway enters into effective conflict with the impulse to escape from the water; and this may depend either upon the relative strengths of the two impulses, or upon some inner conditions that facilitate such conflict or render it difficult; secondly, facility in effecting discrimination between the bright and the dim gangways after the hesitant phase has set in. The rats that learn most quickly would seem to be those in which both these factors are favourable.

At present I incline to the view that X comprises as its chief constituents both of these factors. We may, I think, fairly suppose that the discriminatory act brings into play a high level of cortical function, one which the rat's ordinary life in the cage hardly at all brings into play. It may be, then, that this functioning of a cortical level hitherto latent in the race is a principal factor of the transmitted X. The peculiar situation in which the rat finds itself in the tank may be supposed to force into activity this hitherto latent or unused level of potential cortical function. Something of this sort would seem to occur in all successful educational processes, whether in animals or in men.

SUMMARY.

Twenty-three generations of rats have been trained in the tank to the performance of a specific task. The rats of the successive generations have displayed increasing facility in mastering this task. Whereas rats of the control stocks make on the average about 165 errors (and receive the same number of shocks) before learning to avoid the shock, rats of the 23rd generation of trained stock make on the average only 25 errors; the latter have acquired a greatly increased facility in mastering the task, the increase being measured by the difference between 165 and 25 shocks required for learning.

The average degree of facility shown by any group of rats is in the main a function of their genetic constitution.

In the light of our present knowledge there would seem to be only two ways in which such change of constitution as is shown by the rats of the trained stock can be brought about: first, by steady selection of such variations or mutations as may occur in the direction of such

change; secondly, by transmission of modifications acquired by the rats in the course of training.

It seems very improbable that the former process, selection, can have played any appreciable part in producing the change of constitution here described; and still more improbable that selection can have been the main or the sole process: first, because selection (other than strictly random selection) was carefully avoided by me from first to last in the conduct of the experiment upon the T.R.'s; secondly, because in the stock $W.C._1$ to $W.C._5$ a selection strongly adverse to the increase of facility was exercised on the generations $W.C._3$ and $W.C._4$, and, nevertheless, marked increase of facility was shown by the generation $W.C._5$.

It begins to look to me as though Lamarckian transmission were a real process in nature; and I submit for criticism the proposition that, if continuance of the experiment, combining training with strongly adverse selection, should result in steadily increasing facility, the reality of Lamarckian transmission will have been demonstrated.

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THE TECHNIQUE OF EXPERIMENTATION ON THE PSYCHO-GALVANIC REFLEX PHENOMENON AND THE PHENOMENON OF TARCHANOFF. I.

By ROBERT H. THOULESS.

(*From the Department of Psychology, the University, Glasgow.*)

I. The physical nature of the phenomena.

Two distinct changes of an electrical nature can be measured in a human subject as accompaniments to mental processes or as responses to external stimuli. The psycho-galvanic reflex (P.G.R.) is an increase in the current passing through the body from an external source. The phenomenon of Tarchanoff (T-phenomenon) is a somewhat more complex change in the current which passes between two electrodes on the skin when no external source of current is used.

That the T-phenomenon is a change in the potential difference between the two points of the skin on which the electrodes are placed, there can be no doubt. About the physical nature of the P.G.R. there is more dispute. Gildemeister believes that the application of an external E.M.F. to the skin causes the production of a back E.M.F. of polarization which is comparable in size with the imposed E.M.F. itself, and that the P.G.R. is a decrease in this polarization (1, 2). Einthoven and Bijtel believe, on the other hand, that the part played by polarization in the skin is much smaller than Gildemeister has maintained, and that the true resistance of the skin is relatively large (3). If they are right, there is no reason for supposing that the P.G.R. is anything but a change in skin resistance. There is no simple way of deciding between these alternatives[1]. In a direct current circuit, a decrease of polarization is

[1] It is generally stated by recent writers on the subject that the polarization theory is proved by the fact that Gildemeister showed that when alternating currents are used the resistance of the body is much lower and the P.G.R. is absent (1). This, however, does not settle the matter. Einthoven and Bijtel put forward an alternative theory of the lowering of body resistance to alternating current (3), and Gildemeister's evidence for the polarization theory depends on the use of a much more complex criterion (2). Moreover, even if the P.G.R. is a phenomenon of reduced polarizability, it ought to be present with alternating current although much reduced in amount. In an investigation not yet published, I have found that this expectation is fulfilled.

indistinguishable from a decrease of resistance. Evidence on the matter can be obtained by the use of high frequency alternating currents and also by direct currents of high E.M.F., but the results are difficult of interpretation. In a prolonged experimental investigation, I have convinced myself that Gildemeister's explanation is the true one. It will, therefore, be assumed to be true for the remainder of the present article.

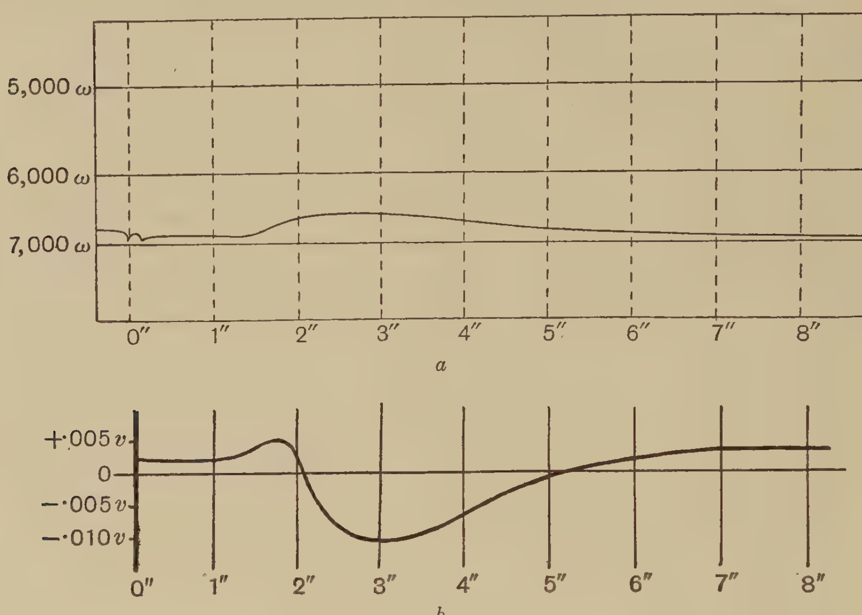


Fig. 1.

a. Photographic record of change in apparent resistance of a subject for 8 seconds after discharge of toy pistol (measured by string galvanometer in a Wheatstone Bridge circuit).

b. Drawing from record of change in somatic E.M.F. of the same subject to the same stimulus in the same series of experiments (showing threefold change in E.M.F. and complete reversal of current during negative change).

The typical form of the P.G.R. phenomenon is shown in Fig. 1 *a*. This was obtained on a string galvanometer in a Wheatstone bridge circuit with an E.M.F. of 1·86 volts between electrodes on the back and palm of the hand (back positive). The stimulus used was the report of a toy pistol. There is a slight increase of current taking place in this particular record in two stages, one at the moment of stimulation and one between 1/10th and 1/5th sec., amounting in all to an increase in apparent resistance of about 100 ohms. This positive change is generally

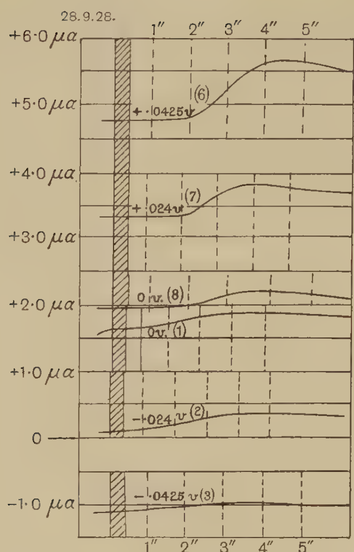
attributed to the electrical effects of muscular contractions. With stimuli less startling than the pistol it is generally absent, although it is sometimes found in the P.G.R. response to any stimulation. From this positive change there is incomplete recovery and the current becomes constant until 1·4 sec. after stimulation when the apparent resistance falls by about 400 ohms to a minimum at 2·8 sec., followed by a recovery which is complete in between 7 and 8 sec. In many hundreds of records with varying stimuli, this curve of decrease in apparent resistance is reproduced without any variation in general form noticeable on casual examination[1], although the latent period is somewhat variable and the time of maximum deflection is more so. The most marked variation of the P.G.R. is the common absence of the initial positive deflection.

One typical form of the phenomenon of Tarchanoff is shown in Fig. 1 *b*. This was reproduced from a record with a string galvanometer obtained at the same sitting as Fig. 1 *a*. No external E.M.F. was used and the current was taken through an external resistance of 10,000 ohms[2]. A deflection above the zero line indicates a direction of current in which the back of the hand is positive and the palm negative (since this is the almost invariable rule, this direction of flow is marked as positive, the other as negative). The initial potential difference is seen to be about 2 mv. After a latent period of 1·2 sec., it rises to 5 mv., then falling to -10 mv. at 3·1 sec., afterwards rising slowly to 4 mv. at 8 sec., falling again slowly to its initial value. The end of the reaction is not shown on this diagram. The recovery is noticeably slower than that of the P.G.R., and may not be complete until about 15 sec. after the stimulation. The latent periods of reactions and the maximum and minimum points are found to be somewhat variable, even when the experiment is carried out under constant conditions. The latent periods have been shown by Gildemeister and Ebbinghaus to depend on the skin temperature (5).

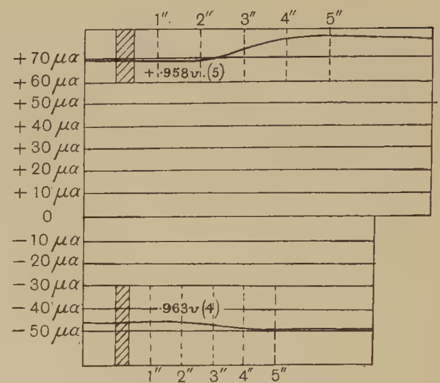
The T-phenomenon is much more variable than the P.G.R. The most striking variation is the replacement of the three-fold reaction shown in Fig. 1 *b* by a simple increase in the current. This form of the reaction is shown in Fig. 2 *A* (2). It can often be seen that, although there is no

[1] There may, however, be significant variations in curvature which would be apparent to a sufficiently careful examination, or which might be shown by the use of Godefroy's *tachogram* method (4).

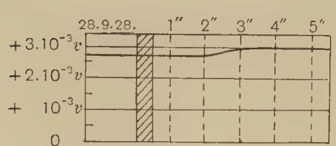
[2] By the principles laid down in Section III, this external resistance would not be large enough to obtain the T-phenomenon undistorted by the P.G.R. if there had been a larger initial deflection. It happened, in this particular experiment, that the initial deflection was very small, so the conditions approximated to those of the Tarchanoff method of neutralizing the initial current.



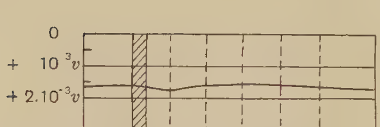
A. Electrical response to stimulation, using small external E.M.F.'s of different strengths (shunt × 10)



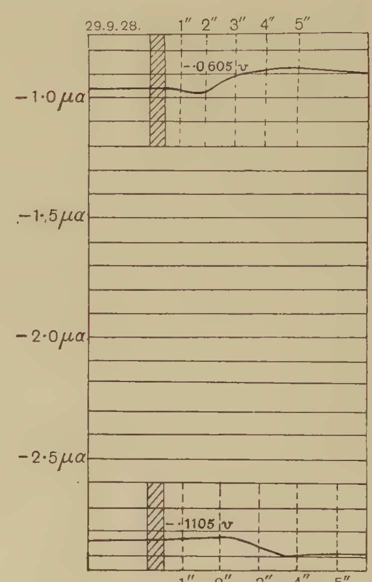
B. As A, but larger external E.M.F.'s (shunt × 300).



Electrodes as above.



C. T-phenomenon with 200,000 ω external resistance (shunt × 1).



D. Part of record like A obtained on a different day, showing reversal of direction of response when external E.M.F. in negative direction is sufficiently increased.

Fig. 2.

change in direction of the deflection in this form of the T-phenomenon, there is a noticeable change of curvature at the point where the negative deflection should appear, which suggests that this reaction is not absent but is simply swamped by the much larger positive change. Weinberg has suggested that the simple form of the deflection is obtained by a subject in a passive condition and the complex form when he is active (6). This is probably part of the explanation of the difference, but I have also obtained both forms of the curve from the same subject in successive observations separated by two or three minutes (for example, in Fig. 2 *C*).

Every investigator of these phenomena has probably at some time considered that both may be exactly the same physical phenomenon measured under different conditions of circuit. A change in potential inside a direct current circuit is indistinguishable from a resistance change. Therefore, the T-phenomenon would produce an apparent change in resistance when we passed a current from an external source through a subject. Conversely, a change in the subject's resistance or polarizability would produce an increase in the current flowing between electrodes on his skin in the conditions we use for measuring the T-phenomenon, even if there were no real change in the skin potential. In other words, the change in apparent resistance in the P.G.R. might merely be the potential change measured in the T-phenomenon, or, alternatively, if the P.G.R. were real, the T-phenomenon might be simply the P.G.R. measured by the E.M.F. of the subject's own skin.

Closer examination shows that these views are untenable and that we are really dealing with two physically distinct phenomena. The evidence for this is:

(1) Each phenomenon can be measured under conditions which make the effect of the other negligible. The methods of doing this will be discussed in the next two sections.

(2) The characteristic difference of form already mentioned. The T-phenomenon frequently shows the three-fold form of Fig. 1 *b*; the P.G.R. never shows this form.

(3) The actual sizes of the two changes is conclusive evidence against their identification. The P.G.R. is commonly a 10 per cent. change of apparent resistance in a 2-volt circuit, while the T-phenomenon is a potential change of less than 10 mv. I have obtained a P.G.R. of 4·5 per cent. in a circuit of 43·5 volts, which if it were due to a potential change would require one more than 200 times as great as the largest T-phenomenon.

Similarly, although the apparent extent of the T-phenomenon must, under many conditions of measurement, be influenced by the P.G.R. phenomenon (for the somatic current itself is influenced by body resistance or polarizability), the T-phenomenon is often far too large to be possibly caused merely by the effect of the P.G.R. phenomenon on the somatic current. Particularly clear evidence of its independence is furnished by the T-phenomenon shown in Fig. 1, for here there is actual reversal of the direction in which the current flows. This could plainly only be caused by a change in potential, for no change in resistance or polarizability could do more than change the amount of the current while leaving its direction unchanged.

The most convincing demonstration of the independence of the two phenomena and of their combined effect on the total current change following stimulation under different conditions of circuit is provided by an experiment in which the current changes to a single stimulus are studied with different small values of external E.M.F. Figs. 2 and 3 show the results of such an experiment.

The stimulus was a loud buzz in a telephone head-receiver from a current interrupted 250 times per second by an electrically driven tuning-fork. A Wundt's time-sense apparatus was used to control the duration of the stimuli which was a little less than $\frac{1}{2}$ sec. The responses were recorded photographically. On the bromide paper was recorded also the period of the stimulus (shown as a shaded area in the photographs) and the time in fifths of a second. The electrodes were Ag/AgCl discs of 21·2 cm.^{2}, placed one on the back, the other on the palm of the subject's left hand[1]. The external current was supplied from an accumulator, connected with a potential divider and a commutator for reversing its direction. The galvanometer was a Moll suitably shunted.

The somatic current was found (as is usual) to flow through the external circuit from the back to the palm of the hand. I have, therefore, adopted the convention of indicating a current or an E.M.F. in this direction as positive, and in the other direction as negative. An external E.M.F. of + 0·0425 volt means, therefore, one in which the anode is on the palm of the hand, the kathode on the back (*i.e.* one which produces a current in the same direction as the somatic current).

Fig. 2 *A* shows the total responses obtained with values of external E.M.F. varying from + 0·0425 volt to − 0·0425 volt. The photographic records have been traced with Indian ink for clear reproduction. The external E.M.F. is written over each curve, while the serial order of the

[1] Details of electrodes will be given in Section IV.

different observations is shown in brackets. No refinement of the method of administering stimuli can make the response an absolutely constant one, but it will be seen that there is sufficient constancy for the general tendency of the results to be clear. The P.G.R. is proportional to the current strength. The total response is, therefore, largest with 0·0425 volt, where a large P.G.R. is acting in the same direction as the T-phenomenon. As the external E.M.F. is lowered the total response becomes less, and is least at − 0·0425 volt where the P.G.R. and T-phenomenon are acting in opposite directions. The response is, however, still in a positive direction because the P.G.R. response is smaller than the T-phenomenon.

In Fig. 2 *B* are shown the responses obtained in the same series of experiments, with E.M.F.'s of nearly 1 volt strength. These cannot be shown on the same scale as the others because the currents are so much larger. If they were magnified to the same scale, it would be seen that the current changes are also very much larger: + 9·8 micro-amperes for the upper one and − 5·6 μ amp. for the lower, while the largest deflection in Fig. 2 *A* is + 0·87 μ amp. The P.G.R. is now very much larger than the T-phenomenon, and (as would be expected) the deflection for the lower curve in Fig. 2 *B* is in a negative direction since the P.G.R. has much more than neutralized the small positive change of the T-phenomenon.

Somewhere, therefore, between − 0·0425 and − 0·963 volt external, the positive deflection of the T-phenomenon has been exactly neutralized by the opposite P.G.R. change. To see more precisely where this change took place, another series of experiments was performed, in which a larger number of small negative E.M.F.'s were used. Two of the results are shown in Fig. 2 *D*. It will be seen that with − 0·0605 volt, there is a positive deflection; the T-phenomenon predominates although reduced in amount by the opposing P.G.R. With − 0·1105 volt, on the other hand, the deflection is negative; the P.G.R. predominates although reduced by the T-phenomenon.

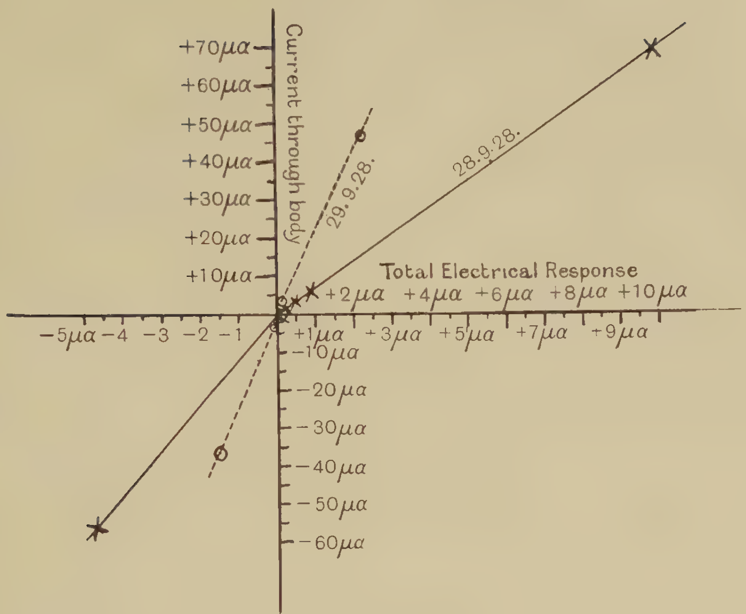
It will be noticed that the whole series of results entirely negatives the statement made by Prideaux that the P.G.R. is absent with small external E.M.F.'s (8). The P.G.R. is present so long as any current flows through the body. Nor is Prideaux's result to be expected on theoretical grounds (on the polarization hypothesis any more than on the resistance hypothesis). A small current produces, it is true, a small back E.M.F. of polarization, but not one which is a smaller fraction of the polarizing E.M.F. The effect on apparent resistance of a change in polarizability will be no less if a small measuring current is used than with a large one. Actually it is the use of a very large current that reduces the P.G.R.

Fig. 2 *C* shows another way of measuring the T-phenomenon in nearly a pure form. A resistance of 200,000 ohms has been put in series with the subject, so that changes in the apparent resistance of the subject (which are only of the order of 2000 ohms) may be rendered of negligible effect in comparison with changes in somatic E.M.F. The initial E.M.F. shown in the upper curve (+ 0·026 volt) is in close agreement with that calculated from the voltage needed in Fig. 2 *A* to reduce the deflection to zero. The T-phenomenon is 0·003 volt, which again is roughly the same as that indicated in Nos. 1 and 8 of Fig. 4 *A*.

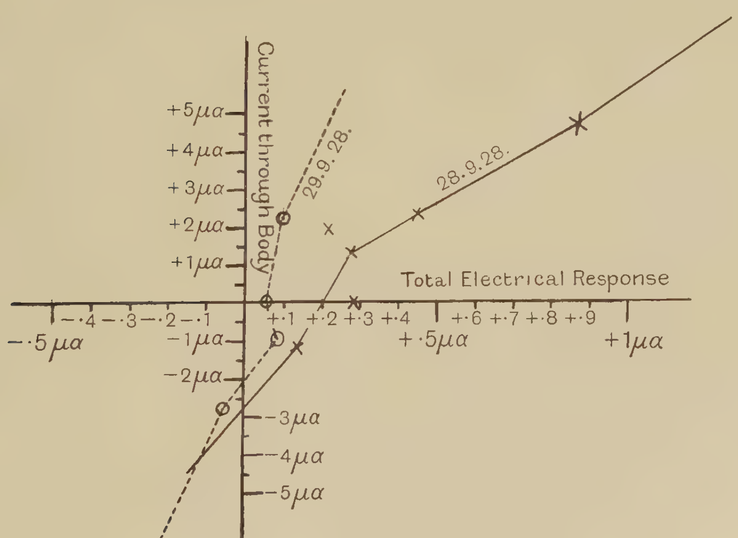
To make sure that the initial current was really a somatic current and did not flow from the electrodes, the position of the electrodes was reversed, the one previously on the back of the hand was placed on the palm, and *vice versa*. The E.M.F. was also found to be reversed, showing that it was mainly from the hand. Its new value was, however, only 0·016 volt. This difference may be due to the electrodes showing a potential difference of 0·005 volt (an unimportant amount), or it may be due to the unavoidable disturbance of the subject. The new T-phenomenon now shows the complex form shown in Fig. 1.

In Fig. 3 these results are shown as a graph in which is plotted the total electrical response to stimulation (*i.e.* the current change due to the P.G.R. and to the T-phenomenon together) at various values of current through the body (*i.e.* current due to the imposed E.M.F. together with the somatic current). Again we have the difficulty that the extreme values, when the external E.M.F. was in the neighbourhood of 1 volt, cannot be shown on a scale which is large enough adequately to represent the smaller values. Fig. 3 *A*, therefore, shows the relation of these extreme values to the others, while Fig. 3 *B* shows the relationship of the values obtained for small external E.M.F.'s on a scale too large to include the extreme values. The points marked by crosses and joined by a continuous line are the values for the responses shown in the photographs of Figs. 2 *A* and 2 *B*, while those shown by circles and joined by a broken line are the complete series of which two members have been shown in Fig. 2 *D*.

If the T-phenomenon were the only electrical response, the curves in Fig. 3 would be straight lines parallel to the base (the change in current would be a constant amount and would not vary with the current strength). If the P.G.R. were the only response, the curves would be straight lines inclined to the axes but passing through the origin (the change in current would be proportional to the current strength). If both are present, the curves will be straight lines inclined to the axes



A. Curve of variation of electrical response with initial current.



B. Central part of A shown on a larger scale.

Fig. 3.

but not passing through the origin. They will cut the horizontal axis at a distance approximately equal to the magnitude of the T-phenomenon alone. While there are inconsistencies between individual readings, which are due to the fact that both phenomena are variable in quantity, it is clear that it is to this last condition that the curves approximate[1]. There is a T-phenomenon which is the main effect when the current through the body is small, while there is also a P.G.R. effect which is approximately proportional to the current strength. While the T-phenomenon may be presumed to remain the same in absolute amount whatever the current strength, its magnitude relative to the P.G.R. effect becomes less as the current strength is increased until at a sufficiently large current strength it becomes negligible. In the next two sections, these results will be used to determine what are the conditions of circuit under which each phenomenon can be measured in a pure form.

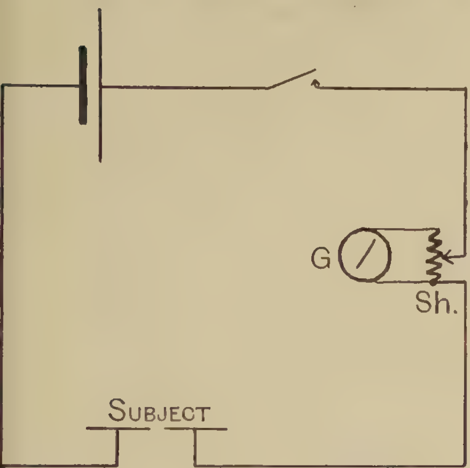
II. The measurement of the psychò-galvanic reflex.

For many purposes it makes no difference whether we accept the 'resistance' or the 'polarization' hypothesis. There is, however, a set of practical problems in the work of the experimental psychologist in which the difference between these two hypotheses cannot be altogether ignored. This is the whole range of problems in which we measure the apparent resistance of the subject or its changes, with the intention of comparing magnitudes obtained under one set of conditions with those obtained under another set of conditions. If we wish to make such comparative measurements, we cannot afford to ignore the question of whether we are, in fact, making an absolute measurement of a resistance or whether we are dealing with a mere appearance of resistance.

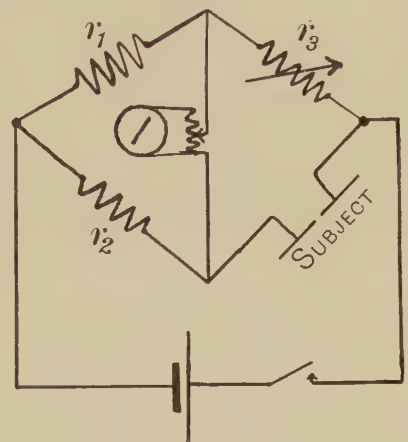
It must be remembered that we never directly measure resistance in a physical experiment; we measure currents and their changes. Even on a Wheatstone bridge in which we have balanced the subject on the fourth arm of the bridge (Fig. 4 *B*), the quantity $r_2 . r_3/r_1$, which we call his 'resistance,' is in reality a measure of e/i, where e is the E.M.F. applied to the electrodes and i is the resulting current flowing between them. It is much more obvious that we are not directly measuring a resistance if the measure is made in a straight circuit without a bridge (Fig. 4 *A*).

[1] The curve showing the variation of current change with current strength is not exactly a straight line, because with very high external E.M.F.'s (20–40 volts) I find that the P.G.R. measured as percentage change of current becomes measurably less. This I believe to be due to a given change in polarizability affecting actual polarization less as saturation of polarization is approached. This effect is, however, very small and may be neglected when e is 2 volts or less.

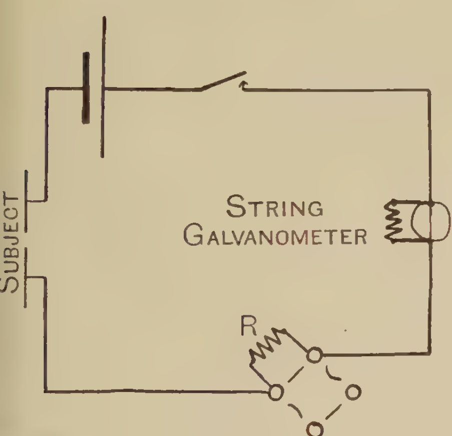
If any polarization is present, the current flowing through the subject will be $(e-p)/W$, where p is the back E.M.F. of polarization and W is the true resistance of the subject. The quantity measured as his 're-



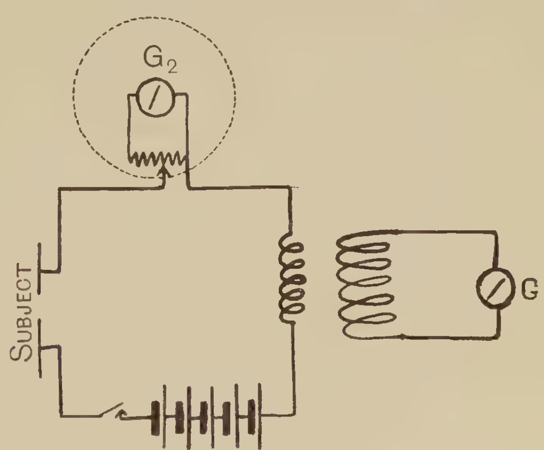
A. Straight circuit for P.G.R.



B. Wheatstone bridge circuit.



C. Salomonson's circuit.



D. Godefroy's circuit.

Fig. 4.

sistance' is in reality e divided by $(e-p)/W$, *i.e.* it is $e.W/(e-p)$. Let us call this quantity the 'apparent resistance' of the subject, and use for it the symbol W'.

If there were no polarization (so e/i was a direct measure of W), we could be content to measure e/i and state simply that using electrodes

of a certain size on a certain part of the subject's body, we had found that his resistance was, let us say, 11,000 ohms. This quantity would be an absolute measurement of a simple physical fact. But on no theory (neither Gildemeister's nor Einthoven's) is this the case. W' is a quantity which will be different when measured in different ways. We cannot be content to say that the apparent resistance is 11,000 ohms because the fraction $e.W/(e-p)$ will have a value dependent on the strength of current used in measuring it. With larger E.M.F.'s, p becomes a smaller fraction of e, and the apparent resistance is thus decreased. This means that a mere statement of W' is always insufficient. We must also state the value of e, or of i, or of both E (the E.M.F. of the battery used) and also the total external resistance in the circuit.

One of the most serious consequences of the realization that W' is not a resistance and that $\Delta W'$ (the change in apparent resistance) is not a resistance change is the consequent uncertainty as to what is a significant numerical measure of the extent of the reaction for different values of W'. Let us suppose that we are doing a word-reaction experiment. We call out the word 'dog' and our subject's apparent resistance changes from 10,000 ohms to 9000 ohms. Ten minutes later when we come to the word 'cat,' his apparent resistance is 7000 ohms. What change of resistance would indicate that his reaction to 'cat' was equal to that to 'dog'? Commonly it is assumed that such equality would be shown by a change of 700 ohms (*i.e.* by an equal percentage drop in apparent resistance), but for no sufficient reason. If $\Delta W'$ were a resistance change, it would be a reasonably certain assumption that $\Delta W'/W'$ would be constant for equal reactions, but if the change is really one of polarization and $\Delta W'$ is a somewhat complicated function of the change of polarization, it ceases to be certain that $\Delta W'/W'$ is the significant measure of the P.G.R. On the polarization hypothesis, this is a serious difficulty, gravely limiting the probability of comparisons between the extent of the P.G.R. at different apparent resistances being significant. The problem of what is a significant measure of the P.G.R. is not yet solved. The question will be further discussed in a later section.

It remains true that we must express our results somehow. It is practically convenient to use e/i as a measure and to call it the 'apparent resistance,' and to express the P.G.R. as $100 \,.\, \Delta W'/W'$, and to call this 'the percentage change in apparent resistance.' There is no harm in this so long as it is realized that it is no more than a practically convenient device with no precise physical meaning.

A further complication in the measurement of W' is indicated by the curves shown in Fig. 2. When measuring the resistance of our subject

with an external E.M.F., the potential difference between the electrodes is not simply that imposed by the external battery. There is also an E.M.F. of somatic origin whose changes are measured in the T-phenomenon. This E.M.F. we may call ϵ. The current through the subject will be $\{e + \epsilon - p_{(e+\epsilon)}\}/W$, where $p_{(e+\epsilon)}$ is the back E.M.F. of polarization produced by the combined action of e and ϵ.

We must either measure ϵ and make the necessary correction in our formula for calculating W', or else make e so large that ϵ is negligible in comparison. If, for any reason, we want to measure W' with small values of e the former course must be adopted. It is better to avoid it, however, if possible, since ϵ is a highly variable quantity so such a correction produces an element of unreliability in the results obtained. For most practical purposes, it is better to make e so large that ϵ may be ignored.

To get some idea of how large e should be for this purpose, we may calculate from the results given in Fig. 3. These, in fact, are very suitable for the purpose since the value of ϵ is unusually large.

The true value of W' will be $(e + \epsilon)/i$; if, however, we ignore ϵ, we shall take W' as e/i. The error involved is therefore measured by ϵ/e. In Fig. 2, ϵ was 0·025 volt. The error in the measurement of W' with an external E.M.F. of n volts would therefore be $2{\cdot}5/n$ per cent. For No. 5 in which nearly 1 volt external was used, the uncorrected value of W' is 13,900 ohms; corrected for ϵ, it is 14,300 ohms. For No. 4, W' is 20,200 ohms uncorrected, 19,700 ohms corrected[1]. It is very rarely in practice that we want to determine W' with such precision that an error of 2 or 3 per cent. is of any importance. So far as ϵ is concerned, we can be content with any value of e greater than 1 volt. If we use a much smaller voltage than this, we must be prepared for a correspondingly large (and variable) error. A more important consideration limiting the smallness of e is the necessity of seeing that P.G.R. measurements are not appreciably affected by changes in ϵ, *i.e.* that the measurement of the P.G.R. is not affected by the influence of the T-phenomenon. This also is secured by making e sufficiently large, since the change in current of the P.G.R. is roughly proportional to the absolute current through the body (*i.e.* roughly to e), while the current change of the T-phenomenon is independent of e.

Again, we can turn to the measurements shown in Fig. 2 as an indi-

[1] It will be noticed that, even after this correction, a very large difference remains between the two values of W' obtained by external currents in opposite directions. This is a real difference which is always found, and has nothing to do with the error in measurement produced by ϵ.

cation of how great will be the influence of the T-phenomenon on the P.G.R. at different values of e. The top record of Fig. 2 *D* shows a T-phenomenon of about 3 mv., that indicated by No. 2 of Fig. 2 *A* is between 3·5 and 4 mv. No. 4 of Fig. 2 *B* shows a P.G.R. of 10·55 per cent., No. 5 of 11·55 per cent.

Let us consider the effect of a T-phenomenon of 4 mv. on a P.G.R. of 11 per cent. To a very close approximation, a change in somatic E.M.F. of $\Delta\epsilon$ will simulate a change in apparent resistance of $100 \,.\, \Delta\epsilon/e$ per cent. The measured P.G.R. will be increased by that amount if the external current is passed through the hand in a positive direction, and correspondingly diminished if the external current is in a negative direction. We can reasonably be content with an elimination of the effects of $\Delta\epsilon$ which makes these effects not more than a twentieth of the P.G.R. With an external E.M.F. of 1 volt, the fraction $100 \,.\, \Delta\epsilon/e$ becomes 0·4 per cent. Since the measured P.G.R. is about 10 per cent., this is less than one-twentieth of the P.G.R., and the T-phenomenon may be regarded as satisfactorily eliminated if the measurement of the P.G.R. is made with an external E.M.F. of 1 volt. A reasonable margin of safety is given if we double this value and measure the P.G.R. with an e of 2 volts[1].

These figures have, of course, no universal validity, being derived from one set of measurements. They may be regarded as a rough but practical indication of the conditions of safety of P.G.R. circuits with electrodes on the back and palm of the hand. With electrodes in other positions, or for any P.G.R. measurements of high precision, the influence of ϵ and of $\Delta\epsilon$ should be determined by measurement and calculation.

The opposite problem of how the T-phenomenon may be measured with satisfactory elimination of the effect of the P.G.R. will be discussed in the next section.

A final point which must be considered here is the practical effect of the dependence of both W' and $\Delta W'$ on e. On no hypothesis is W' an absolute measurement; on Gildemeister's hypothesis, $\Delta W'/W'$ also is not. We shall be safe in saying that both measurements are functions of the measuring E.M.F. If any comparative use of P.G.R. results is to be made, the control of e is imperative.

[1] This refers, of course, to the actual potential difference between the electrodes, not to the E.M.F. of the battery used. If large external resistances are introduced into the circuit, e will be considerably smaller than E, and with a badly arranged circuit a battery of 2 or more volts may give no guarantee that e is large enough to eliminate the T-phenomenon. In a straight circuit, $e = E \,.\, W'/(R + W')$, where R is the external resistance. In a balanced Wheatstone bridge with negligible external resistance, $e = E \,.\, W'/(W' + r_1)$.

An idea of the magnitude of the effect of increasing external E.M.F. on W' and on $\Delta W'$ can be obtained from the following table which shows mean values of W' and $\Delta W'$ obtained in successive applications of the same sound stimulus (exactly controlled in intensity and duration) to a subject with Ag/AgCl electrodes of 21·2 cm.^{2} on the back and palm of the right hand (anode on the back).

Table I. *Variation of apparent resistance and* P.G.R. *with external* E.M.F.[1]

e (volts)	W'	$\Delta W'$ (as % of W')
2·08	7630	18·6
22·8	4640	6·4
33·1	3010	3·15

It is probable that the variations of P.G.R. with an e of less than 2 volts are much smaller than those shown in the above table. I have not found it possible to establish significant differences for these lower values, because (a) the differences (if present) are small, (b) the P.G.R. appears to be more variable with low values of e, and (c) the reliability of their measurement is seriously disturbed by the simultaneous presence of the T-phenomenon. It would, however, be rash to assume that variations of $\Delta W'/W'$ with e are absent for low values of e, and we shall be on the side of caution if we treat e as one of the factors that must always be controlled in order to get significant values of W' and of $\Delta W'$.

This leads to a further practical consideration. W' is a measure whose numerical value depends on the E.M.F. used to measure it. Alternatively we can say that it depends on the strength of current i used to measure it. After a P.G.R. has taken place, W' may have altered so much that there will be a considerable difference in i, and in some conditions of circuit in both e and i. We may therefore be measuring W' under different conditions before and after the reaction, so that our measure of W' is partly a product of this difference in conditions.

Both practical and theoretical considerations suggest that the resultant error is not a large one. It can, however, be so easily eliminated altogether that it is worth while to arrange the circuit for this end. What is required is that either i or e should be kept constant (*i.e.* independent of W'). Theoretically, i could be kept constant by having an external resistance in series with the subject so large that $\Delta W'$ was negligible in comparison with it. The E.M.F. from the battery would also

[1] These results are only given as indication of the general results of increase of e. They are means from an insufficient number of observations to be regarded as highly reliable. The general indications of these results were confirmed by a reliable series taken with different electrodes on another part of the hand. These have not yet been published.

be made correspondingly large, and the P.G.R. would be measured by a high resistance galvanometer in parallel with the subject. In practice, this would be a very inconvenient arrangement. The alternative, to keep e constant, is secured very simply by using a straight circuit in which the total external resistance is negligible in comparison with W'. This can easily be arranged by suitably shunting the galvanometer and by keeping all other resistances out of the circuit. A considerable external resistance in a straight circuit is, of course, also objectionable because it reduces the sensitivity of the measurement of $\Delta W'$. The commonly used Wheatstone bridge circuit never fulfils the condition that e should remain the same after a change in W'. This is one objection to the use of the Wheatstone bridge circuit for P.G.R. measurements. Other objections to it will be mentioned later.

After long experimentation with alternative ways of measuring the P.G.R., I have come to the conclusion that the obvious and straightforward circuit shown in Fig. 4 *A* is the best. This includes simply a battery of 2 or more volts, the subject, and a shunted galvanometer. There will be an initial deflection of the galvanometer which will be a measure of the reciprocal of the subject's initial apparent resistance. Changes in this deflection after stimulation give a measure of the change of conductivity due to the P.G.R. The only objection to this method is that the deflections obtained are much smaller than those of the Wheatstone bridge method. This would be a serious practical objection if the P.G.R. were a small change. In fact, however, it is generally of the order of about 10 per cent. and a change of this size can be measured with sufficient accuracy in a direct circuit. For most purposes, there is no advantage in over-refining reliability of measurement since the phenomenon itself shows uncontrollable variations which are a considerable proportion of the whole reaction. We may, without difficulty, make our initial deflection as much as 40 cm.; the P.G.R. change of deflection will then be about 40 mm. and may be read with an accuracy of 0·5 mm. (*i.e.* with an error of the order of 2 per cent.). This assumes that the phenomenon is measured visually. In photographic recording, the deflections must be smaller, but also the accuracy of reading is greater (I have found, in practice, that the error does not exceed 0·2 mm. with good records). With a P.G.R. change in deflection of 10 mm. (which can easily be secured by suitable shunting) this gives an accuracy of the same order.

If a higher accuracy than this is required, or if the P.G.R.'s measured are much smaller than 10 per cent., the investigator is driven to use the Wheatstone bridge method (Fig. 4 *B*) in spite of its many disadvantages.

It is much to be regretted, however, that this is commonly regarded as the standard method of measuring the phenomenon and is taught to students as such.

The physicist uses the Wheatstone bridge for the delicate measurement of an unknown resistance X, which is placed in the fourth arm of the bridge (occupied in the diagram by the subject). An external battery is used and r_3 is adjusted until no current flows through the galvanometer. The value of X is then discovered from the relationship $X/r_3 = r_2/r_1$. This is called a 'null' method of measurement, because no current is flowing through the galvanometer at the point of balance. Since the current will be infinitesimal in the last stages of balance, the galvanometer may be made very sensitive; hence the high precision of this method of measuring resistance.

In making P.G.R. measurements, we place our subject in the position of X, and measure his apparent resistance as $r_2 . r_3/r_1$. It is true that this is not his ohmic resistance but is compounded of his true resistance and a back E.M.F. of polarization. This, however, introduces no new complication here for the W' measured on the Wheatstone bridge is the same as that which would be measured in a direct circuit by the same current.

Serious difficulties arise, however, from the fact that we cannot use a null method to measure a changing resistance. The bridge cannot be balanced both at the beginning and end of the reaction. At best, we can have it exactly balanced at the beginning (although this is often difficult). The deflection of the galvanometer in an unbalanced bridge bears no simple relationship to the change of resistance in the fourth arm, and is also a function of the value of r_3. What then are we to take as a measure of the P.G.R.? To collect varying values of the deflection obtained on an unbalanced bridge even with r_3 constant (more obviously if it is changed during the experiment) and to call these values of the P.G.R. phenomenon, is clearly fallacious. To take the average of such a collection of physically meaningless figures and to call it the mean P.G.R. is worse.

Two courses remain open to the experimenter. He can calculate the value of W' which would give each observed deflection, or he can determine it empirically. The simplest formula for calculation (that in which there is only negligible resistance external to the bridge circuit) is as follows: i (the current through the galvanometer)

$$= \frac{E\,(r_1 W' - r_2 r_3)}{g\,(r_1 + r_3)\,(r_2 + W') + r_1 r_3\,(r_2 + W') + r_2 W'\,(r_1 + r_3)}$$

(where g is the resistance of the galvanometer).

In practice, the best method is, however, to replace the subject by a variable resistance and to measure separately for every P.G.R. obtained the value of this variable resistance which will give the same initial and final deflections in the same conditions of circuit. This is laborious, but less so than calculation from the formula. This is the method which should always be employed when the Wheatstone bridge is used for P.G.R. measurements. It is almost unbelievable that serious investigators have evaluated their results by placing a small constant resistance in series with the subject, have discovered the change in deflection when this is short-circuited with the subject still in the circuit, and have calculated $\Delta W'$ on the assumption that it is a simple function of the change in deflection. Figures obtained by such methods are entirely worthless.

There is a last practical objection to the Wheatstone bridge circuit, which is due to the fact that sudden and unexpected changes in the subject's resistance may occur during the course of an experiment. These may easily drive the galvanometer off the scale or even damage it seriously. A change in resistance which would double the current through the galvanometer on a direct circuit may make it a hundred times as great on a Wheatstone bridge circuit. This limits the central advantage of the Wheatstone bridge circuit (that the galvanometer may be made highly sensitive) just when we need it most: with a string galvanometer, for example, which will not give us very precise values in a direct circuit because we cannot pass a sufficiently large initial current through it to give an initial deflection of satisfactory size.

The case against the Wheatstone bridge circuit is heavy both on theoretical and practical grounds. It remains true that the investigator must sometimes use it, and face the labour of calculation or calibration. There is no case for teaching it to students as the standard method. Its theory and practice are complicated. These complications can only be ignored by using inaccurate methods. The straight circuit, on the other hand, is easy to use and is intelligible to the student who has little knowledge of physics.

Fig. 4 *C* shows a circuit used by Prof. Wertheim Salomonson, with the intention of measuring simultaneously the polarization and the true resistance of the subject (9). An external E.M.F. was used and an external resistance R was introduced and short-circuited in rapid alternation by means of a mechanically driven commutator. The currents i_1 and i_2 in these two conditions of the circuit were measured by means of a string galvanometer. W and p were then calculated from the formulae:

$$i_1 = (E - p)/(W + R), \text{ and } i_2 = (E - p)/W.$$

This method assumes that during the short period of each change of the commutator, p will be unchanged. Gildemeister's work shows, however, that this is not the case. A very considerable effect of p on W' is found with alternating currents even of a period of several thousand alternations per second. In fact, what is measured by Salomonson's method is not W (the true resistance) but simply a value of W' which is less in amount than the W' measured with a direct current to an extent dependent on the speed of the commutator. The method fails in its purpose of measuring W, and does no more than could be done equally well and more conveniently by simply using an alternating current for the measurement of W'.

I have also used a modification of Salomonson's method for simultaneously measuring the P.G.R. and the T-phenomenon. For this purpose, the battery is omitted from Fig. 4 C, and the string galvanometer is made sufficiently sensitive to give good deflections with the somatic current alone. Then $i_1 = \epsilon/(W' + R)$, and $i_2 = \epsilon/W'$. From these formulae the values of ϵ and W' can be calculated along the whole length of the curve. It was found that the method was practicable, although it was laborious and inaccurate, the main source of inaccuracy being the unreliability of the string galvanometer at such high sensitivity. Simultaneous measurement of the two phenomena could probably be better carried out by means of two sets of electrodes on different parts of the skin.

Fig. 4 D shows the circuit recommended by Dr Godefroy (4). A current from an external source is passed through the subject and through the primary of an induction coil. The secondary of the induction coil is connected in series with a sensitive galvanometer G_1. So long as the current in the primary circuit is constant, no current flows through G_1, but an increase of current in the primary circuit causes an induced current to flow through G_1 in one direction, while a decrease produces an induced current through G_1 in the opposite direction. What is measured by G_1 is thus not the current through the subject but the changes in that current. If we can ignore the effect of self-induction in the secondary circuit and the inertia of the galvanometer mirror, the deflection of G_1 is proportional to di/dt. In order to get sufficiently large deflections of G_1, it has generally been found necessary to use an external battery of about 10 volts. In order to record at the same time the absolute value of the current in the primary circuit, a second galvanometer G_2 is sometimes inserted in that circuit. Without this second galvanometer there is no indication of the value of W'.

The advantages claimed for this method are (1) that the galvanometer deflections vary about zero so that there are no adjustments to make during the course of an experiment to keep its movements within convenient limits, and (2) that the curve it gives (the *tachogram*) is of far more distinctive form than the simple P.G.R. curve, and so gives more hope of solving the important practical questions of objective differentiation between different emotional reactions.

I have no practical experience of the use of this circuit, so the following comments are of a purely theoretical order.

(1) The tachogram curve contains nothing that is not present in the P.G.R. curve. Every deflection in the tachogram is present as a curvature change in the P.G.R. If a P.G.R. were obtained with a galvanometer whose deflections were proportional to the current and whose period was negligible, and the inclination of the curve at different times was plotted on a graph, the tachogram would be the curve obtained. On the other hand, changes in curvature are not easily seen, and the practical advantage of Godefroy's method of transforming them into deflections, if they are to be studied, is very great.

(2) While all information contained in the tachogram curve is implicit in the P.G.R. curve, the converse is not true. The tachogram tells us the shape of the P.G.R. curve but not its level; it contains no indication of the absolute value of W'.[1] In an earlier investigation, I tried to show that the absolute value of W' has a certain significance of its own, being partly dependent on the degree of 'alertness' of the subject (10). Since we are not wise in an investigation to cut ourselves off from any source of information about what we are investigating, this consideration alone suggests that the galvanometer G_2 should never be omitted.

(3) There is, however, a more serious reason why a method of measuring W' cannot properly be omitted from this circuit. We have seen that D (the deflection of G_1) $\propto di/dt$. We may usefully carry the calculation further in order to discover how D is dependent on the changes in W'.

$$i = e/W', \text{ and } D = k.di/dt,$$

therefore
$$dW'/dt = \frac{dW'}{di}.\frac{di}{dt} = -\frac{W'^2}{e}.\frac{D}{k},$$

or
$$D = -k\frac{e}{W'^2}.\frac{dW'}{dt}.$$

[1] Because there is no unique solution to the differential equation $x = \int \frac{di}{dt}.dt$.

The tachogram deflection is thus not proportional to the rate of change in W' but to the ratio of this rate to the square of W'. This brings us to a variant of a problem already discussed, that of a significant numerical measure for the P.G.R. If two tachogram deflections obtained at different times with the same e are equal, we are naturally inclined to say that these two reactions (perhaps to two different stimulus words) were equal. But what it means in physical fact is that the maximum value of dW'/W'^2 was the same in each case. W' may have been different, so if some other function of W' had been measured (let us say dW'/W'), the two reactions would have appeared unequal. Clearly we have no reason for supposing that dW'/W'^2 rather than any other function of W' is equal for equal emotional reactions. If we know W', this is a no more serious difficulty with the tachogram method than with the ordinary P.G.R. method. It means simply that we are not sure of any method of making significant comparisons between the sizes of different deflections, unless these have been obtained with equal values of W'. It becomes a fatal objection to any numerical comparison of tachogram figures obtained in a circuit in which G_2 has been omitted so long as W' is unknown. In such a circuit, there is no indication of how W' has varied during the course of the experiment, so the numerical record of the sizes of the deflections is simply meaningless.

There are other possible circuits for the measurement of the P.G.R., as, for example, the Kohlrausch bridge circuit with alternating current used by Gildemeister (1). Such circuits are, however, only useful for special purposes, and are not required in the ordinary course of investigation.

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SUMMATION AND SUBTRACTION OF BRIGHTNESS IN BINOCULAR PERCEPTION

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I. Statement of problem.

Since homonymous halves of both retinae are connected with the same half of the cerebrum, it is usually assumed that corresponding points upon the two retinae outside of the fovea are represented in the visual projection areas of the cortex by single patterns. It is therefore generally accepted that the two retinae function integratively, *i.e.* that the singleness of vision with binocular observation is made possible by the unity of their central connections. Numerous results gathered from observations of stereoscopic phenomena such as retinal rivalry support the view that alteration of conditions upon one retina always exerts some influence on the functioning of the other retina. Consequently it is rather surprising that Fechner[1], Sherrington[2], Abney and Watson[3], Dawson[4] and others have claimed to have proved by experiment that there is no binocular summation of brightness, or, in other words, that though the eyes function integratively for other conditions they have little appreciable antagonistic or facilitative influence upon one another as far as brightness is concerned. To be sure, other investigators, such as

[1] See v. Helmholtz, *Handbuch d. Physiol. Optik*, 3rd ed. II, 287.

[2] C. S. Sherrington, *Integrative Action of the Nervous System*, pp. 354–93.

[3] Abney and Watson, *Phil. Trans.* A, 1916, CCXVI, 109.

[4] Dawson, this *Journal*, 1917, VIII, 510.

McDougall,[1] and some earlier men, like Valerius[2], Piper[3] and Aubert[4], claimed to have found a slight summation effect. However, as the weight of argument from the standpoint of numbers of investigations, prestige of investigators and use of refined apparatus is decidedly against it, binocular integration as regards brightness has come to be a generally discredited fact in the literature[5].

It was the purpose of this investigation to attempt to put this problem of binocular summation of brightness to a critical test, and in particular to determine if possible why the experimental results should be so flatly contradictory. As Sherrington's investigation was the most elaborate one and represents a direct attack upon the problem, we will begin by referring to certain important items in his results.

II. Unexplained results from Sherrington's investigation.

A careful perusal of Sherrington's article in which he claimed to have refuted the notion that there is binocular summation of brightness will reveal a number of candid statements in which he admits that small amounts of summation were observable. For example, on p. 364[6] we read: "The binocular arrangement, then, is said by them to require a slightly higher frequency for extension of flicker than does the uniocular.... (p. 365). From these observations it appears that similar phases of flickering illumination, if timed to fall coincidently on conjugate retinal areas, do *very slightly reinforce* each other in sensation, and, if timed exactly alternately, do *very slightly mutually reduce*. But the *broad outcome of the observations is that*, so far from bright phases at one eye effacing dark phases at the corresponding spot of the other eye, *there is hardly a trace of any such interference*." On p. 372 we find this: "In certain instances the binocular combination did appear just distinctly the brighter....But there occurred frequent instances in which no excess was observed in the brightness of binocular combinations over that of their carefully balanced uniocular components."

We do not of course reproduce these excerpts in order to cast any suspicion upon Sherrington's conclusions. His conclusions follow logically from his results. In the next section, however, an attempt will be made to sum up the reasons why Sherrington obtained no conclusive results favouring summation and why we did obtain conclusive results.

[1] W. McDougall, *Mind*, N.S. 1901, x.
[2] Valerius, *Poggendorff's Annal.* CL, 17.
[3] Piper, *Stach. f. Psychol. u. Physiol. d. Sinnesorg.* 1904, XXXII, 161.
[4] Aubert, *Physiol. d. Netzhaut*, Breslau, p. 287.
[5] J. H. Parsons, *Colour Vision*, pp. 57, 142.
[6] *Op. cit.*

III. PRECAUTIONS NECESSARY IN AN INVESTIGATION OF THIS NATURE.

1. In the first place we should like to point out that an individual is a very poor judge of relative brightnesses[1]. On account of the handicap which subjects suffer in judging brightnesses under ordinary circumstances, we concluded that the ideal apparatus to use for this problem ought to possess two comparison surfaces, one of which might be varied uniocularly and the other binocularly. That is to say, we wished

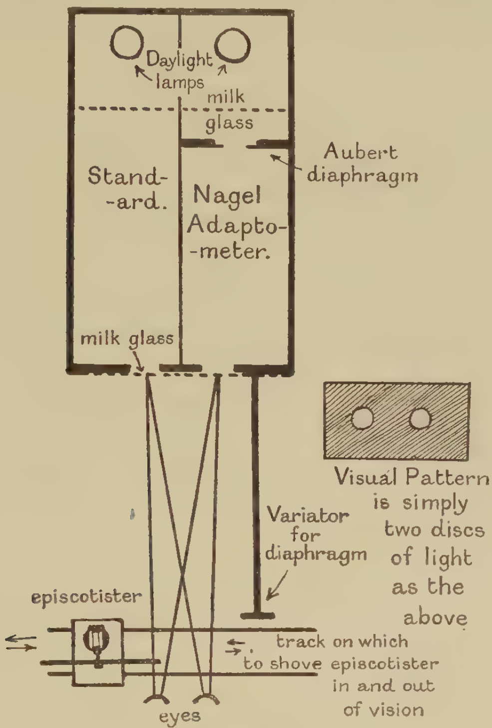

Fig. 1

[1] In *Photometry*, Van Nostrand, 1926, J. W. T. Walsh emphasizes the neglected fact that "The enormous range of sensitivity possessed by the eye can readily be appreciated from the fact that the ratio of the brightness of objects seen by direct sunlight to that of the same objects seen by starlight on a clear moonless night is at least 10 million to 1.... This power of adapting itself to different conditions of lighting makes the eye useless for the direct measurement of luminous energy, and, as has been said already, photometry depends on the eye solely for the determination of the equality of two adjacent fields as regards either brightness or contrast." The 'slight' subjective differences of brightness such as those reported by Sherrington's subjects might have been found to be of considerable value in spite of the observers' evaluations of 'very slight,' if the experimental conditions had been such that they could have been measured.

to arrange a situation in which the light coming from one field to one eye could be varied and measured and that at the same time light from a standard field which always reached both eyes could be altered. In this way, the observer would always have a standard present by which to make his judgment. We constructed the apparatus represented in Fig. 1 to satisfy these conditions.

2. Statistical methods were introduced in order to overcome errors produced by adaptation, induction and other undetermined variables. Sherrington discovered that 'in certain instances' binocular combinations appeared brighter and that in other instances which he concluded were more frequent, the binocular combinations did not appear so bright. We wished to obtain a measure of the frequency of this predominance so as to be able to say something definite regarding the average instance. Accordingly, we made hundreds of observations and averaged the results.

3. We foresaw that summative effects might be more pronounced with different ratios of uniocular and binocular illuminations and so, after a preliminary experiment to determine the most critical positions, we adopted the following nine ratios of reduction of light: 1/16, 1/8, 1/4, 1/2, 3/4, 15/16, 47/48 and total[1].

4. We decided to perform our first experiment in the dark in order to offset simultaneous contrast influence of background upon the figure[2]. Adoption of dark surroundings also aided in obscuring the presence of the episcotister blade which sometimes causes a displacement of the observer's depth fixation and produces a filmy shadow effect. Another advantage of using dark surroundings is that it reduced variations in pupillary sizes to a minimum. Under the circumstances, artificial pupils were unnecessary.

[1] As the Talbot Law was at one time supposed not to work with low intensities, we tested its accuracy under our experimental conditions by comparing our results with those obtained by cutting down the illumination with Bausch and Lomb neutral tint filters with ratios of 1/2, 3/4, and 7/8. We found that the dimming effect was practically the same with the filters as with the episcotister. Hyde, working at the Bureau of Standards, found that the law applies accurately over a wide range of physical intensities. Cf. Hyde, *Bull. of Bureau of Standards, Washington, D.C.* II, 1, 1906.

[2] We are working on the summative and subtractive effects with lights of higher intensities at the present time. Binocular summation does seem to hold equally well here as with low illuminations.

IV. PROCEDURE.

The nature of our procedure of investigation was as follows: The observer first attempted to equate the brightness of the two lighted fields by turning a handle to vary the brightness of the right-hand field which was controlled by the adaptometer. After making an equation, an episcotister disc rotating at a velocity well above the critical frequency for fusion was brought in from the left just far enough so that it came between the observer's left eye and the left-hand illuminated field. The observer's view of the right-hand field through his left eye was always unimpeded[1].

Episcotister discs of the different reduction ratios listed in the first column of Table I were employed in irregular order. After the left-hand object had been dimmed somewhat by interposing the episcotister blade, the observer was asked to equate the two fields by altering the right-hand field which was seen by both eyes. Invariably it was found that he diminished the right-hand field in order to equate it to the left-hand field which was dimmed upon one eye only. The proportion of diminution of the left-hand field by the episcotister was measured and the results are diagrammatically represented in Fig. 2. After 25 observations were made, the observer changed places with the experimenter. When two periods of 25 observations were completed, a rest interval was introduced and the subjects left the room and became daylight adapted. One hundred double equations were gathered for each reduction ratio listed in the first column of Table I. Consequently the data for the five series depicted in Table I represents 8400 separate observations for each observer.

The advantage of this procedure over other procedures for studying binocular integration is unquestionable. Many of the earlier observations were made by the method of successive comparison of brightness, which, on account of the delay between presentations to be judged, is extremely difficult, and is never used in photometric work on account of its crudeness. Other investigators have employed a method of simultaneous comparison with a stereoscope, where the observer was asked to report upon the combined effect of two identical superimposed fields which differed only in brightness. This method of studying binocular effects is subject to considerable inaccuracy because, unless a standard is common to both eyes, there is no accurate way by which to make a simultaneous discrimination between the combination of two dissimilar uniocular

[1] A study is being made at the present time of left-eye and right-eye differences.

effects and the binocular integration of two similar effects, except upon the basis of memory. Our apparatus and procedure allows such a

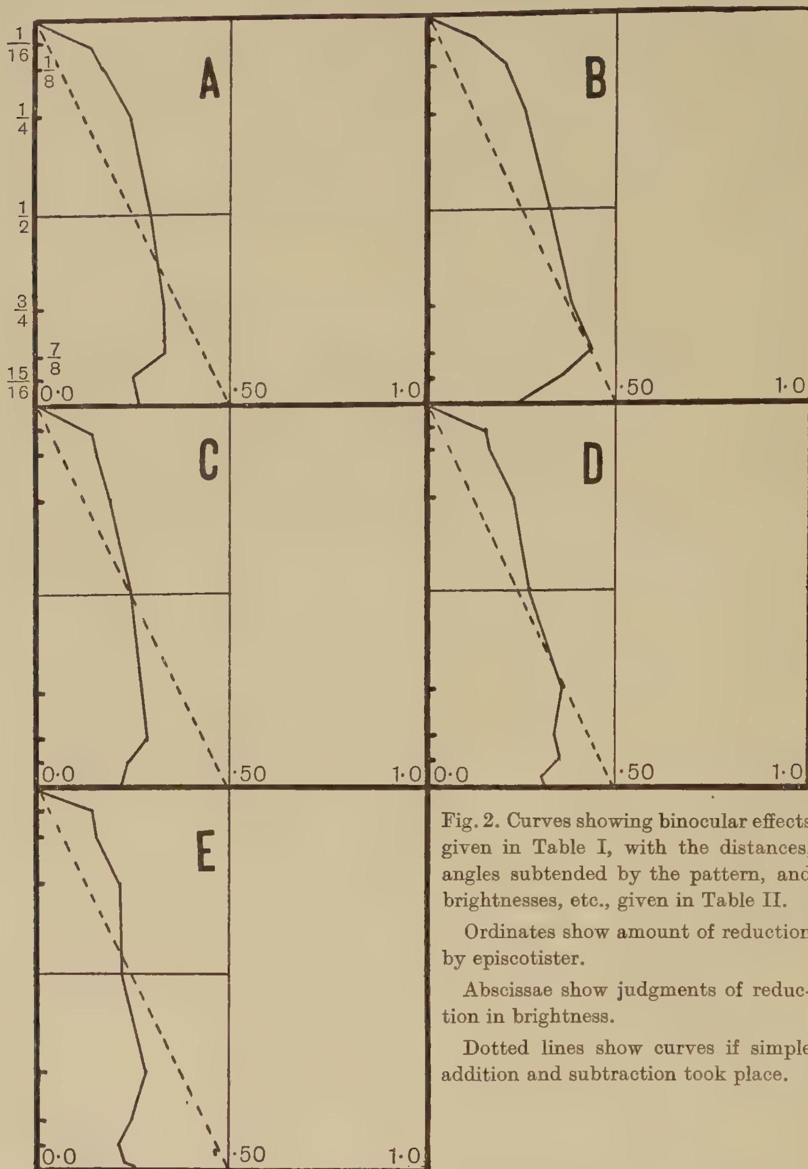

Fig. 2. Curves showing binocular effects given in Table I, with the distances, angles subtended by the pattern, and brightnesses, etc., given in Table II.

Ordinates show amount of reduction by episcotister.

Abscissae show judgments of reduction in brightness.

Dotted lines show curves if simple addition and subtraction took place.

binocular standard for comparison and represents a considerable advance over the successive procedure method or the stereoscope procedure

method which is ordinarily a successive procedure method also, since it depends upon memory.

Casual observations of the subjects bear out this emphasis upon the importance of our procedure. We found that when the light coming from one field to one eye was dimmed that there was a difference in brightness between the two fields, but we were not at all certain as to how much difference there was. In fact, sometimes we were not positive that there was any difference. However, as soon as we began to change the illumination of the field which was undimmed to both eyes we became subjectively much more certain that there was a difference in brightness between the two fields and that we could adjust for this discrepancy. It was found that our equations for the brightness of the two fields when both were undimmed varied up and down from observation to observation, but that the adjustments made when one was dimmed by the episcotister also varied up and down in parallel fashion. Thus, by making an equation afresh each time, we cancelled out the uncontrolled general level fluctuations in sensitivity of the two eyes.

Table I.

Reduction	A	B	C	D	E
1/16	0·174	0·172	0·148	0·156	0·140
1/8	0·188	0·208	0·162	0·166	0·165
1/4	0·243	0·257	0·190	0·229	0·213
1/2	0·293	0·304	0·248	0·282	0·229
3/4	0·317	0·372	0·279	0·359	0·280
7/8	0·317	0·428	0·282	0·339	0·265
15/16	0·251	0·366	0·239	0·354	0·211
47/48	—	—	—	0·300	0·239
Total	0·276	0·258	0·217	0·305	0·258

Table II.

Case	Distance cm.	Angles subtended Disc	Pattern	Disc area	Flux lumens	Brightness millamberts	Retinal illumination in photons
A	147	2′ 5″	10° 56′	31·2	0·00285	0·091	2·10
B	147	2′ 5″	10° 56′	31·2	0·00570	0·182	4·06
C	147	2′ 5″	10° 56′	31·2	0·01425	0·455	9·78
D	71	5′ 6″	23° 00′	31·2	0·01425	0·455	9·78
E	420	51′ 4″	2° 33′	31·2	0·01425	0·455	9·78

We checked the results of our own observations at various points with the results of casual observers. We found that the results from two trained observers in psychology and the results from a trained astronomer, who was accustomed to judge brightness of stars, correlated closely with our own results.

V. Results.

Fig. 2 represents graphically the composite results of two observers who are the authors of this paper. As their results varied only in minor details, we are representing the average for the two observers in the form of composite curves.

The curves *A*, *B*, *C* summarize the results obtained at different levels of illumination. Thus, by referring to Table II we see that in Case A the retinal illumination in photons was approximately 2·10. In Case B it was 4·06, and in Case C it was 9·78. Cases B and C were approximately double Cases A and B. Curves *D* and *E* give the results obtained from varying the size of the retinal pattern. By referring to Table II, we see that in Case D the two fields were far apart so that the total distance amounted to 23° in terms of the visual angle subtended. In Case D, however, the total distance between the fields was small (they both fell within the rod-free area) as the visual angle subtended was only 2° 33′.

In no case, it will be noted, does the psychological subtractive effect parallel the subtractive effect of the stimulus. With low degrees of dimming of physical intensities, the subtractive effect of brightness is relatively greater. With high degrees of dimming of the physical intensities, the subtractive effect of brightness is relatively less. The maximal absolute subtractive effect occurred when the physical intensity was reduced from 3/4 to 7/8.

Table I gives the averaged data from which the curves in Fig. 2 were plotted.

VI. Bearing of results upon Fechner's paradox and upon F. Allen's reflex effects.

Fechner's paradox is an observation to the effect that if the uniocular images differ in brightness the binocular brightness becomes considerably less than the brighter of the uniocular components. This subtractive effect was supposed to reach a maximum when the ratio of the two brightnesses was as 1 to 25. When, however, the ratio of the two brightnesses was as 1 to 1·5, the binocular brightness was said to be about equal to that of the brighter component. We believe that our statistical results are more accurate than those reported from the crude method usually employed in studying Fechner's paradox. According to our results, the maximal absolute subtraction occurred when the ratio of brightnesses was as 1 to 8 or 1 to 15. Our results do, however, corroborate the common observation that cutting off most of the brightness

of a field has a greater absolute binocular subtractive effect than cutting out one field entirely.

On p. 248 we reported the observation that the summative and subtractive effect of different intensities of light upon each eye was present predominantly in consciousness only when there was a relative change (rise or fall) of brightness upon one eye. In this connection, we also observed that the task of equating the two fields after the darker field had been lightened was very much easier than the task of equating the two fields before the left field had been darkened. In other words, the organism seems to be more sensitive to a binocular summative effect than to a binocular subtractive effect.

We believe that Fechner's paradox is merely a special case of the absolute subtractive effect noted in our study. There are, however, relative effects, both summative and subtractive. There is, to be sure, no absolute arithmetical summation although, with low degrees of cut off, the observed effects are relatively greater than they should be if increases in psychological brightness were correlated perfectly with increases in physical intensity.

Our findings regarding the subtractive effect also harmonize with the important work of F. Allen[1] upon the reflex or induction effect of one eye upon the other as far as chromatic vision is concerned. He found as we did that the maximum absolute 'reflex' effect of one eye upon the other was obtained with very dim light and not with maximum dark adaptation. He showed clearly that the effect of stimulating one portion of one retina with a coloured light not only influenced neighbouring portions but also tended to depress the sensitivity of the opposite retina for light stimuli throughout the whole range of the spectrum.

VII. SUMMARY.

1. We have attempted to prove that there is a certain appreciable and measurable change in the binocular brightness observed when the illumination to one eye is altered.

2. Certain passages were taken from Sherrington's investigation upon this subject to indicate the inconclusiveness of his investigation. Important precautions are enumerated to show why Sherrington did not obtain results in agreement with those obtained in this study. Contrary to the conclusions of Sherrington, we maintain that the two eyes do function integratively as regards brightness, although we concur

[1] A number of papers have appeared from Prof. Allen's laboratory, but most of his results may be found summarized in the *J. of Opt. Soc. of America*, XIII, Oct. 1926.

with him that increments and decrements of intensity cannot be perfectly correlated with increments and decrements of brightness.

3. We have pointed out that studies of binocular brightness should be carried out with an apparatus made to expose a standard field at all times to both eyes. Moreover, this standard should be of such nature that it can be varied and the variation measured in photometric units. In this way, when the light from the variable object to one eye is altered the standard field can be altered by the observer and a direct simultaneous measure of the difference in brightness accurately made.

4. Fechner's paradox is found to be a special case of the subtractive effect. In corroboration of Fechner's effect, we found that the maximal subtractive effect occurs with dim light and not with complete extinction.

5. Our results agree perfectly with the significant findings of F. Allen upon induced effects upon one retina resulting from stimulation of the other retina with coloured light, or with dark adaptation.

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MAXIMUM CAPACITY AND AVERAGE ACHIEVEMENT.

By S. Wyatt.

I. Maximum rate of working as a standard of measurement.

(*a*) *The problem.*

In almost all machine-feeding operations, output can be readily expressed as a percentage of the possible maximum. The latter is a definitely fixed standard which depends on the speed of the machine. In the numerous manual processes in industry, no such standard exists, and output is usually expressed in terms of average rate of working together with some measure of variability about this average. These values fail to show the extent to which activity is depressed by unfavourable elements associated with the conditions of work, and it is suggested that maximum achievement under the most favourable conditions, *i.e.* when the operative is actuated by suitable incentives and is free from fatigue, should be used for this purpose. The difference between maximum capacity and average achievement in the spell of work would then be a measure of the influences which are detrimental to productive activity.

(*b*) *Unit chosen.*

Obviously, the longer the period during which work is continuously performed, the lower will be the average rate of working in that period. The choice of a period to represent maximum attainment will be a

somewhat arbitrary procedure, but must be long enough to neutralize the effect of a phenomenal spurt or errors of measurement, yet short enough to avoid the effects of fatigue or a weakening of the incentive to work. A period of 5 minutes seems to conform most closely to these requirements, since in that time it is usually possible to complete several units of output without feeling bored or fatigued. Providing the same unit of measurement is used when different individuals or processes are compared, the choice of 5, 10 or 15 minutes is relatively unimportant.

(c) *Reliability of unit chosen.*

Maximum performance in a 5-minute period appears to be very consistent from day to day. This belief is based on the results obtained from a number of industrial operatives engaged in making cigarettes by hand. Each operative was asked to work as quickly as possible at a time coinciding with the period of highest output (as shown by the work curves). The results obtained by adopting this procedure on five different occasions are given in Table I.

Table I. *Number of cigarettes completed by each worker during a 5-minute period when working at maximum speed.*

Worker	1st test	2nd test	3rd test	4th test	5th test	Average
A	23·0	23·5	23·3	24·0	23·2	23·4
B	23·5	23·0	23·9	23·2	23·9	23·5
C	27·0	27·0	27·5	26·5	27·1	27·0
D	25·0	24·2	25·8	26·0	25·2	25·2
E	26·2	28·0	26·8	27·4	26·2	26·9
F	28·5	27·2	29·4	28·1	28·7	28·4
G	27·5	28·1	28·0	27·2	27·6	27·7
H	24·0	25·0	25·5	24·8	24·1	24·7
I	24·5	23·5	25·0	24·8	24·3	24·4
J	28·5	30·3	29·5	30·0	29·2	29·5
K	28·8	28·5	28·7	28·8	29·5	28·9
L	24·0	22·5	23·2	23·0	23·7	23·3
M	30·2	29·8	29·0	30·2	28·5	29·5
N	21·0	21·5	22·6	22·1	22·8	22·0
O	23·2	23·0	23·8	23·3	23·0	23·3
P	26·2	27·0	26·5	26·7	26·0	26·5
Q	25·2	24·7	25·6	25·0	24·8	25·1
R	30·0	31·0	31·9	30·3	31·2	30·9
S	24·0	24·2	23·3	24·0	23·7	23·8
T	21·8	20·0	21·0	20·5	21·2	20·9

The results for each worker are very consistent, and indicate that the averages given in Table I are fairly representative of the maximum rate each worker is capable of attaining. This view is supported by the results obtained in a longer series of observations which was limited to six of the operatives included in Table I. Such results are given in Table II.

Table II. *Average number of cigarettes completed by each worker during a 5-minute period, together with the standard deviation of the separate readings from their average.*

Worker	No. of observations	Average	Standard deviation
A	29	23·2	0·56
K	20	28·9	0·61
L	26	23·2	0·57
Q	29	24·9	0·69
S	29	23·8	0·39
T	29	20·9	0·64

The results show that the average is not appreciably changed by an increase in the number of observations, but, if anything, becomes more variable as the series is prolonged. Thus a few readings appear sufficient to establish a reliable measure of maximum achievement.

II. Applications to industrial procedure.

(a) *Individual differences.*

The susceptibility of different individuals to similar conditions of work will be shown by the differences between maximum and average rates of working. Such results, obtained from the cigarette-makers, are given in Table III, and represent:

(i) The maximum rate per 5-minute period as given in Table I.
(ii) The average rate per 5-minute period throughout the day.
(iii) The difference between the maximum and average rates.
(iv) This difference expressed as a percentage of the maximum rate.

Table III. *Differences between maximum and average rates of working.* (*Cigarette-making.*)

Worker	Maximum rate	Average rate	Difference	
			Absolute	Relative
A	23·4	17·3	6·1	26·1
B	23·5	18·4	5·1	21·7
C	27·0	22·1	4·9	18·3
D	25·2	18·0	7·2	28·6
E	26·9	19·2	7·7	28·6
F	28·4	20·9	7·5	26·4
G	27·7	19·7	8·0	28·9
H	24·7	20·0	4·7	19·1
I	24·4	18·5	5·9	24·2
J	29·5	22·3	7·2	24·4
K	28·9	22·6	6·3	21·8
L	23·3	18·9	4·4	18·9
M	29·5	22·8	6·7	22·7
N	22·0	17·7	4·3	19·5
O	23·3	18·7	4·6	19·7
P	26·5	19·3	7·2	27·2
Q	25·1	18·9	6·2	24·7
R	30·9	24·9	6·0	19·4
S	23·8	17·5	6·3	26·4
T	20·9	16·4	4·5	21·5

The above results show that the absolute difference between maximum and average rates of working in the case of individuals exposed to similar conditions of work varies from 4·3 to 8·0 units while the relative difference ranges from 18·3 to 28·9 per cent. In this way operatives may be graded according to the extent to which they are affected by adverse influences associated with the productive process.

It is also apparent that the absolute difference between maximum and average rates of working tends to be greater in the quicker than in the slower workers. The coefficient of correlation (ρ) is + 0·65. Since, however, there is a general tendency for variations about a quantity to increase as the magnitude of the quantity increases, the correlation in question will probably be influenced by this tendency. The correlation between maximum rate of working and relative deviations from the maximum is still positive, though small, ρ equals + 0·16, but in other processes slightly higher coefficients have been obtained, viz.:

	ρ
Examining and packing (10 workers)	+ 0·84
Tobacco-weighing (10 workers)	+ 0·56
Soap-wrapping (6 workers)	+ 0·37

It accordingly appears justifiable to conclude that the quicker workers tend to be more susceptible than the slower operatives to unfavourable elements in the conditions of work.

At the same time it is evident that, in cigarette-making, a fairly close relation exists between the two series representing maximum and average rates of working. The coefficient of correlation (ρ) is + 0·88, consequently, in this process, those individuals who are capable of attaining the highest speeds also tend to produce the highest output under ordinary industrial conditions, and *vice versa*. In the other operations lower coefficients were obtained, viz.:

	ρ
Examining and packing...	+ 0·68
Tobacco-weighing	+ 0·34
Soap-wrapping	+ 0·26

(*b*) *Occupational differences.*

The maximum rate of working may also be used for the purpose of determining and comparing the total effect of adverse influences in different industrial processes. In this case group averages will be used, and the difference between the maximum and average rates of working

in each group expressed as a percentage of the maximum. The following results (Table IV) illustrate the application of this procedure to different industrial processes.

Table IV. *Relative differences between maximum and average rates of working in different industrial processes.*

Process	No. of workers	Maximum rate	Average rate	% decrease
Cigarette-making	20	100	76·6	23·4
Examining and packing	10	100	83·4	16·6
Soap-wrapping	30	100	70·1	29·9
Tobacco-weighing	10	100	62·4	37·6
Rug-making	8	100	88·1	11·9

Subject to the limitations due to the small number of individuals in each group, the results show that the deviation from the maximum was least in the rug-making process. In this operation the operatives were under close supervision and the spell of work was 3 hours as compared with 4 hours in the other processes. The examiners and packers worked closer to their maximum than the cigarette-makers, presumably because the former occupation was more varied and consequently less boring or fatiguing than the latter. The large deviations from the maximum in the case of the tobacco-weighers was undoubtedly due to the fact that they were paid on a time-rate basis while the other workers were paid according to output produced. Soap-wrapping, in which the deviations from the maximum were also large, was a fairly strenuous operation since the workers had to keep pace with a conveyer. Talking was also difficult because of machinery noises, consequently boredom and fatigue were rather pronounced.

The above results give some indication of the total effect of negative influences in different industrial processes, and the method enables occupations to be classified according to the extent of these influences.

(c) *Analytical considerations.*

Differences between maximum and actual rates of working throughout the spell of work provide a basis for determining the nature and magnitude of particular factors which are detrimental to productive activity. The possibilities in this connection may be illustrated by reference to the results obtained in cigarette-making and tobacco-weighing (Fig. 1). These refer to individual workers and have been selected so as to illustrate the type of results obtained in the two processes.

In both curves, the highest rate of working attained by the operative is represented as 100 per cent., and the actual output recorded in

consecutive 5-minute periods is expressed as a percentage of this maximum. The shaded area in each curve accordingly represents the total effect of factors which are detrimental to productive activity.

The most noticeable feature of the curves is the difference between the maximum and the highest rates of working observed under ordinary conditions of work. In cigarette-making, the operatives at some time or other during the day usually approached their maximum speed, but the highest observed rate in tobacco-weighing was generally well below

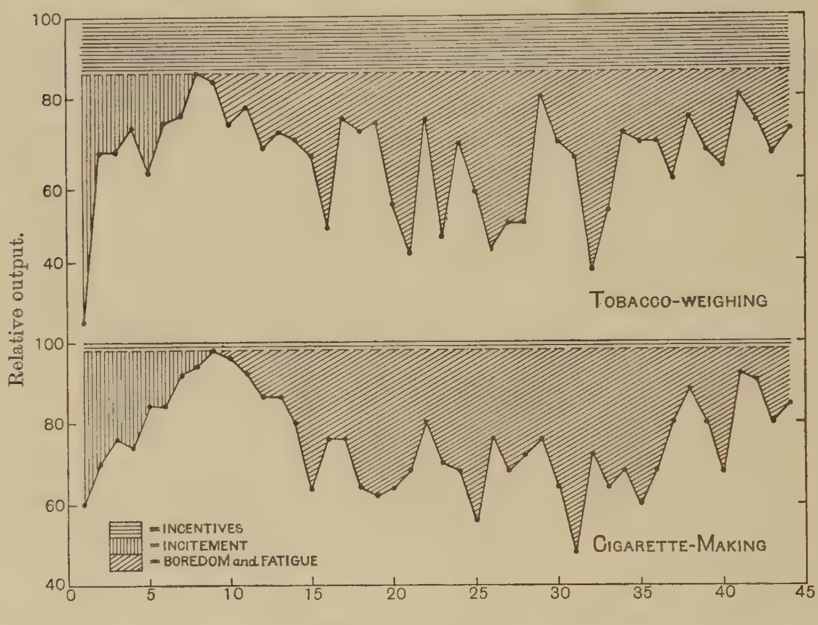

Fig. 1. Illustrating deviations from maximum efficiency and their probable causes.

the maximum. The most probable explanation of this difference appears to be connected with the monetary incentive in the two processes. The cigarette-makers were obviously anxious to earn as much as possible, while the tobacco-weighers seemed to work in a very leisurely manner. Thus, in this case, the general incentive to work is dependent upon the system of payment, and the weakness of this incentive is roughly proportional to the horizontally shaded areas in the curves.

The deviations from the broken line drawn through the highest point on the curve represents the effect of adverse influences of a temporary character. One of these is usually attributed to the process

of 'warming-up' or incitement and is represented approximately by the vertically shaded area. The remaining deviations from the highest level of output recorded during the spell (obliquely shaded area) are usually explained in terms of boredom and fatigue. The variously shaded areas accordingly represent the approximate effect of the different negative incentives to activity, which, when expressed numerically as a percentage of the total loss, are as follows:

	Monetary incentives	Incitement	Boredom and fatigue	Total
Cigarette-making	2·0	3·6	19·3	24·9
Tobacco-weighing	14·0	3·7	17·4	35·1

These values, although given as illustrations of analytical possibilities, are typical of the results obtained in the cigarette-making and tobacco-weighing processes.

III. Discussion of results.

The validity of the arguments and inferences contained in this article depend upon the reliability of the measure of maximum achievement and its use as a standard of reference. Maximum performance over short intervals of time is a function of the psycho-physiological mechanism. It depends upon the ability of the mechanism to translate effort into work. In any individual, there is a fairly definite limit to momentary achievement. Thus a man may be able to register a pull of 100 lb. on a spring-balance, or to remember the names of six articles after seeing them flashed on a screen, and a repetition of the procedure at daily or weekly intervals will, in the practised individual, yield approximately similar results. In many respects an individual is like a machine driven from a storage battery, which, when fully charged and in the comparative absence of frictional and electrical resistance, is capable of producing maximum results. As the voltage falls, or resistances increase, the rate of working is also reduced. Significant deviations from the maximum rate are, however, in one direction, consequently the maximum, unlike the average rate of working, is exposed only to downward variations, and its reliability as a standard of measurement is correspondingly increased.

In general, therefore, maximum performance when recorded in the absence of fatigue and kindred factors is likely to be a fairly consistent and representative measure, and the results presented in Tables I and II support this view.

The applications of the procedure advocated in this article have

some connection with the problem of measuring fatigue. As soon as psychology began to be treated experimentally attempts were made to discover a test for fatigue, and numerous psychological and physiological devices have been suggested for this purpose. The usually accepted definition of fatigue, as causing a diminished capacity for work, has tended to create the impression that all work-decrements are due to fatigue. Other factors, such as boredom and weak incentives have, in consequence, been comparatively neglected. Variations in the rate of working are usually the resultant of several positive and negative factors, and fatigue, although important, is not necessarily supreme. The total effect of adverse influences is shown by the difference between maximum capacity and average achievement, and this measure, whether applied to similar or different conditions of work, fulfils some of the requirements of a test for 'fatigue.'

When used in connection with individuals engaged in the same industrial process, it shows the variations in response to similar conditions of work, and provides a basis for further enquiry into the causes of these differences. It has been said, for instance, that the difference between maximum and average rates of working tends to be greater in the quicker than in the slower workers. This may be due to the fact, which has been noted during numerous investigations on repetition work, that the slower operatives are, as a rule, distinctly conscious of their disability, and attempt to overcome it by the expenditure of relatively greater effort. They are affected by the pace set by the quicker workers, while the latter, because of their superior speed, are more inclined to relax when such inclinations arise. It is highly probable, therefore, that the slower workers suffer more from fatigue than the quicker operatives, yet output curves may suggest the opposite tendency.

The differences in question may also have some connection with the amount of energy expended per unit of time by the quicker and slower workers. Worker R, for instance, was capable of producing 30·9 units while worker N produced only 22·0 units, and it seems reasonable to suppose that 30·9 cycles of activity consume more energy than 22·0 cycles. If this view is correct, worker R will expend more energy per unit of time than worker N (other things being equal), and, in consequence, will become relatively more exhausted as work proceeds[1]. That is, the deviations from maximum capacity will tend to be greater in the case of the quicker operatives.

[1] This is undoubtedly true in the case of the *same* individual when working at higher speeds.

The relation between maximum capacity and average attainment throughout the spell of work will depend upon the relative strength of the incentives to activity and upon ability to endure the conditions of work. This relationship was found to vary in different processes, and was highest in the cigarette-making operation. The operatives in question were the keenest and most industrious group of workers observed over a period of 10 years. They were older than the average women employed in industry and seemed particularly anxious to earn as much as possible. When all the operatives in a group are actuated by strong incentives the possibilities of a close relation between maximum and average rate of working are correspondingly increased.

The tobacco-weighers, who received a time wage, represent the other extreme in this respect. While some were comparatively industrious, others appeared to produce just enough to enable them to keep their jobs in a department controlled by a particularly charitable foreman. Deviations from the maximum were accordingly much greater in some cases than in others, and the low coefficient (0·34) reflects this state of affairs.

As regards the soap-wrappers, the low correlation obtained (0·26) appears to be due to a number of causes. In the first place the workers, who were comparatively young, were taken separately to an adjoining room when being tested for maximum efficiency and some of the best operatives seemed to be adversely affected by this procedure. In the other processes maximum capacity was measured under ordinary conditions of work. Secondly, the slower workers talked less and took fewer voluntary rests than the quicker operatives and consequently deviated relatively less from their maximum speed. Thirdly, a few of the physically weaker operatives were less able to endure the additional strain imposed by trying to keep pace with the conveyer.

A broad survey of industrial processes along the lines suggested in this article would disclose factors of general interest and importance, and indicate the most profitable lines for further research. In the course of time it should be possible to determine the normal amount of deviation to be expected in different forms of industrial activity so that whenever this amount is exceeded the causes can be ascertained.

A standard of possible achievement is a necessary prelude to the determination of the particular factors responsible for reduced efficiency. Such an analysis, when treated quantitatively, is necessarily somewhat speculative, since it is often difficult to say where one factor begins and another ends. Since, however, the results of recent experiments have

increased our knowledge in this direction, the possibilities of accurate measurement have been correspondingly increased.

One of the chief problems in modern industry is the provision of conditions which will facilitate the economical release of energy, and the degree of approximation to such conditions will be shown by the difference between maximum capacity and average achievement.

IV. Summary.

1. It is suggested that the difference between maximum capacity recorded under the most favourable conditions and average achievement observed throughout a spell of work is a measure of the combined influence of factors which are detrimental to productive activity.

A suitable measure of maximum capacity is the highest rate of working attained in a period of 5 minutes. This value is very consistent in the case of the same individual observed at corresponding times on different days.

2. The application of this method to industrial procedure can be used to show

(*a*) Individual susceptibility to similar conditions of work.

(*b*) The total effect of unfavourable factors in different industrial processes.

(*c*) The approximate influence of particular factors responsible for reduced activity.

3. When applied to the processes considered in this article, there is evidence to show that

(*a*) One worker may be almost twice as susceptible as another to unfavourable elements associated with the conditions of work.

(*b*) The quicker workers tend to deviate more than the slower operatives from their respective highest speeds.

(*c*) An imperfect relation exists between maximum and average rates of working, yet when all the workers in a group are actuated by a powerful incentive the degree of imperfection may be very slight.

(*d*) The difference between maximum and average rates of working may vary from 12 to 38 per cent. in different processes.

(*e*) A time-rate system of payment is much less effective than a piece-rate and may cause a reduction in output amounting to approximately 12 per cent.

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CHRISTIAN AND JEWISH CHILDREN IN EAST-END ELEMENTARY SCHOOLS

SOME COMPARATIVE MENTAL CHARACTERISTICS IN RELATION TO RACE AND SOCIAL CLASS

By W. H. WINCH.

I. How the work came to be done.

In a decennial report on the schools of Administrative District 6, to the Elementary Education Committee of the London Education Authority, Christmas, 1923, I wrote as follows:

> A word or two, however, must be given to the Jewish child population, which in this district is so considerable. Many, if not most of the schools attended by Jewish children are in poor neighbourhoods, in what, indeed, are usually called 'slums.' But the Jewish children do not, with rare exception, show the usual characteristics of the slum child: they are well-grown, well-fed, well-clothed and intelligent children, in intellectual advance, age for age, of Christian children of the same social class. I say this, not only from ten years' inspectorial experience in this district, but also from the results of some carefully given tests of the higher mental functions in both Jewish and Christian children. The Jewish girls are, however, in rational functions, at a further distance below the Jewish boys than are Christian girls from Christian boys; the sexes appear, intellectually, more differentiated. Their general superiority, age for age, may be due to the earlier pubertic period of the Jewish child or to intellectual racial superiority: probably both factors operate; and Jewish mothers, be it remembered, do not, with rare exceptions, 'go out to work.'

I had not then made a sufficiently intensive attack on the influence which the varieties of 'social class,' even within the same school, may have had on the intellectual and scholastic functions of the children. I was aware that, in some schools, attended jointly by Christian and Jewish children, the Christian children came from poorer streets and poorer homes, and were, distinctly, of lower social standing. But I knew, or thought I knew, of the schools in which they did not. So I began, in the autumn of 1926, some intensive work on this particular issue.

II. The problem stated.

Having now had some 14 years' inspectorial experience in East-end London Schools, with the results and papers of the London County Council Preliminary Examination for Junior County Scholarships coming before me every 6 months, and hearing the frequently expressed views of the teachers, I could scarcely have refrained from arriving at some more or less reasoned opinions about the relative characteristics of Jewish and Christian children. Further, for some 20 years past, I have given sets of my own 'reasoning tests' to whole schools in different parts of London the results of which, from time to time, have lent support to the opinions of the experienced teachers who had, or had had, charge of schools of Christian or Jewish children or of Jewish and Christian children mixed. Those opinions, confining myself to intellectual characteristics, were and are: (1) that Jewish children, age for age, are more intelligent than Christian children; (2) that Christian children are somewhat more proficient in manual work than Jewish children; (3) that there is a greater difference between the mental powers of the best and the worst of the Jewish than there is between the best and the worst of the Christian children; (4) that the Jewish girls show bigger differences from Jewish boys than Christian girls from Christian boys. For some years past, I had given sets of my reasoning tests in East-end schools, among others, and there is no doubt that, in schools attended by Jewish children, the results are decidedly higher than those for Christian children in adjacent, or even in what appear to be the same neighbourhoods. It became obvious, at quite an early stage of my inspectorial work, that often, when Jewish and Christian children attended the same school, the Christian children were generally of poorer and lower social type than the Jews. In school E, for example, the Christians came from two very poor and mean streets in the immediate vicinity of the school, where no Jewish children lived. In school F, the Jewish children came from the Whitechapel side of the school, and the Christians from St George's-in-the-East, and there, too, the differences of ability were so marked that they were a matter of general comment. Further examples of this kind could be given. But it may well be asked, "Are there not schools in which the whole of the pupils in attendance are Jewish, why not compare them with schools in which all the pupils are Christians?" The issues are not so simply decided. For, no scholastic proficiencies, no 'mental or intelligence tests' are uninfluenced by teaching, and it would be necessary to have the same or equal teachers if the results

were to have more than an inspectorial value. We must have the pupils, if we are to compare them satisfactorily, race by race, age by age, taught in the same school, with the same syllabuses of instruction, by the same methods, class by class, and by the same teachers. We may, of course, indeed, we often do, in a rough practical way, say, "Such a Christian school is of the same social class as such a Jewish school, and the teaching assessments of the staff are approximately equal, what are the comparative results in this or that 'mental test' or test of attainment in these conditions?" But one can never feel any real security; for if, as I believe, a close equality in instruction and training are necessary before the results of any 'mental test' can have exact comparative value as to natural ability, we must avoid these large-scale observations in which, of necessity, the relevant differences, other than those with which we are specially concerned, remain unknown and obscured.

III. What is meant by the term 'social class'

By 'social class' I mean something rather more inherent than 'home circumstances' though less inherent than race. Some of the criteria which I shall employ *are* criteria of home circumstances. But 'he ain't no class' is a current term of reproach which has no direct relation to prosperity or poverty. Some investigators have used the rental of the family as an index number, which has yielded valuable results. Some have used the occupation of the parent as a guide. It is believed that Jewish families are larger than Christian families within the same social ranks. Some of the children may be at work and contributing to the family exchequer. A high rental with a low figure of individual accommodation may indicate a lower social class than a smaller rental and a smaller family. There is little doubt that good social class does lead ordinarily to a relatively high payment for housing accommodation, more than for clothing or food. The occupation of the parent is useful also, but it is not decisive. So I obtained through the head teachers, and the experienced members of their staffs, and the knowledge of a local attendance officer, particulars of family rental, the number of rooms occupied, the number of adults living in them, the number of children living at home, the occupations of the parents, and those other indications of 'class' or 'status' which are known to teachers and to local workers. Then the head master of the boys' and the head mistress of the girls' school made independent estimates, giving the marks, 4, 3, 2, 1, or 0 according to the aggregate of the social class conditions.

Since many of the children had brothers and sisters attending the same school, there was an opportunity of collaboration and comparison in the marks allotted to the same and other families.

There is one important issue which needs mention. A larger proportion relatively, year by year, of Jews than Christians had obtained scholarships, attended central schools (which happens at 11 plus years of age), or had become fee-paying pupils at secondary schools.

IV. The tests, their administration and marking.

I used some sets of my own reasoning tests as indications of natural rational intelligence[1]. The tests used in these two schools were Sets *A* and *E*. The methods of marking the problems of Set *A* are fully set out in the *Journal of Experimental Pedagogy* for December, 1921. Examples of the answers, and the marking of the problems of Set *E* have not yet been published, but the questions have been much used over a period of years, and modified in accordance with the results from time to time. Every paper was marked by me for both sets of tests in both schools, with a *viva voce* reference in every case in which the child's apparent meaning was not clear. The second set of tests was given one week after the first. As a measure of manual dexterity, we used the handwriting which was done in writing out the answers to the questions in reasoning. The marks for each set were 4, 3, 2, 1, or 0 both in reasoning and handwriting.

V. The value of school classes and school gradings.

A valuable means of estimating the 'standard' or 'grade' of scholastic attainment will be found in the school grading into standards, of which there are seven, the seventh being the highest. They depend on an aggregate of marks in terminal examinations, and not on intelligence tests. Let us first see what light is thrown on the relative abilities of Christian and Jewish boys and girls when the school standards, their ages, their reasoning marks, their writing marks, and their marks for social class are compared in sets of averages.

[1] "The Transfer of Improvement in Reasoning in School-Children," this *Journal*, April 1923, XIII (4).

VI. Results.

Table I. *Showing school standards of Christians and Jews, their ages, reasoning and writing marks, and marks for social class compared.*

Boys.

School standards	No. of boys C.	No. of boys J.	Av. age in years and months C. y. m.	Av. age in years and months J. y. m.	Av. mark for reasoning C.	Av. mark for reasoning J	Av. mark for handwriting C.	Av. mark for handwriting J.	Av. mark for social class C.	Av. mark for social class J.
VII	9	21	13 4	12 6	3·28	3·55	2·67	2·71	2·22	2·71
VI	15	21	12 6	11 10	2·97	2·90	2·67	2·74	2·40	2·62
V *a*	14	25	11 11	11 3	2·71	2·80	2·71	2·86	1·57	2·32
V *b*	12	15	12 1	11 3	2·17	2·10	2·63	2·30	1·75	2·87
IV	21	15	10 11	10 3	1·74	1·93	2·38	2·43	1·86	3·00
III	14	19	9 9	9 5	1·03	1·66	1·25	1·79	2·00	2·84
II	16	14	9 3	8 0	0·63	0·61	2·03	1·86	1·63	3·07
I	17	16	7 7	7 8	0·03	0·16	0·88	1·22	1·53	2·44

C. = Christians. J. = Jews.

It is evident that in standard of attainments, the Jewish boys are considerably in advance, age for age, of the Christian children, for they have reached their respective standards, as a rule, indeed almost invariably, at an earlier age, in some cases at a much earlier age. The marks for reasoning show a close correspondence between the Jewish and Christian boys within the same school standards, as do also those for handwriting, as they should do. In social class the Jews show a marked superiority, and the question may very fairly be raised whether the whole difference in standard of scholastic attainment may not be due to the factor of social class rather than that of race.

Let us now cut across the classification by school grading, and classify the boys according to age-groups.

Table II. *Showing age-groups of Christians and Jews, their reasoning and writing marks, and marks for social class compared.*

Boys.

Age-groups in years	No. of boys C.	No. of boys J.	Av. age in years and months C. y. m.	Av. age in years and months J. y. m.	Av. mark for reasoning C.	Av. mark for reasoning J.	Av. mark for handwriting C.	Av. mark for handwriting J.	Av. mark for social class C.	Av. mark for social class J.
14	3	3	14 0	14 4	3·17	3·00	2·50	2·67	1·00	2·33
13	16	18	13 5	13 6	2·97	2·75	2·84	2·25	2·06	2·39
12	24	19	12 3	12 4	2·46	2·76	2·50	3·00	1·92	2·42
11	16	22	11 5	11 5	1·59	2·57	2·59	3·00	1·38	2·56
10	19	28	10 5	10 5	1·71	2·59	2·27	2·30	2·00	2·89
9	9	19	9 5	9 5	1·50	1·92	1·78	2·18	2·00	2·74
8	15	18	8 5	8 5	0·57	1·11	1·40	1·86	2·07	2·94
7	15	17	7 6	7 6	0·13	0·32	1·00	1·38	1·67	2·71
6	1	2	6 11	6 11	0·50	0·75	1·00	0·75	2·00	3·50

Nearly all the boys of 14 had left the school, and of the normal group of 12 and 13-year old children, some of the abler children, predominantly Jews, had obtained scholarships to secondary schools or were attending central elementary schools. Of the 7-year old group, a very few were in the infants' department; and, of course, only a very few of the abler 6-year old children had been promoted from the infants' school. With these facts in mind, it is apparent that the Jewish boys have scored in reasoning, age for age, all along the line. They are also rather better writers; but they are much superior in social class in every age group. And again the question arises, if social class is a factor in these results, may not the whole superiority exhibited by the Jewish boys be due to this factor, and not at all to race?

Let us now turn to the girls' department of the same school, which is, of course, a separate school in conduct and organization.

Table III. *Showing school standards of Christians and Jews, their ages, reasoning and writing marks, and marks for social class compared.*

Girls.

School standards	No. of girls C.	No. of girls J.	Av. age in years and months C. y.	C. m.	J. y.	J. m.	Av. mark for reasoning C.	Av. mark for reasoning J.	Av. mark for handwriting C.	Av. mark for handwriting J.	Av. mark for social class C.	Av. mark for social class J.
VII	5	13	13	2	13	2	2·50	3·04	2·80	2·85	2·40	2·62
VI	7	24	12	2	12	3	2·86	2·29	3·21	2·83	2·43	2·63
V *a*	19	17	11	10	11	8	2·50	2·53	2·69	2·88	2·00	2·47
V *b*	15	17	12	3	11	4	2·43	2·44	2·63	2·21	1·40	2·41
IV *a*	13	25	11	6	10	5	1·81	2·22	2·85	2·96	1·23	2·32
IV *b*	18	15	11	0	10	2	1·78	1·90	2·42	2·20	1·44	2·40
III	12	21	10	1	10	0	0·93	1·33	2·42	2·33	1·33	2·57
II	18	18	8	3	8	3	0·58	0·89	2·69	2·39	1·89	2·06
I	16	19	7	5	7	4	0·09	0·26	1·84	1·95	1·25	1·79

Again we see that in standard of scholastic attainment, measured by their school grading, the Jewish children are in advance of the Christian children, for they have, almost invariably, attained their school standards at an earlier age, in some cases at a much earlier age. The marks for reasoning show, except in the case of the girls of Standard VI, a balance of advantage in favour of the Jews, even though the children are in the same scholastic standards. In handwriting the balance of advantage oscillates; it can hardly be said that there is any superiority either way. In social class the Jewish children show a marked superiority all along the line.

Let us now classify in accordance with age-groups and disregard the school grading into standards of attainment.

Table IV. *Showing age-groups of Christians and Jews, their reasoning and writing marks, and marks for social class compared.*

Girls.

Age-groups in years	No. of girls		Av. age in years and months		Av. mark for reasoning		Av. mark for handwriting		Av. mark for social class	
	C.	J.	C. y. m.	J. y. m.	C.	J.	C.	J.	C.	J.
14	0	2	—	14 1	—	3·00	—	2·50	—	2·50
13	16	20	13 7	13 7	2·22	2·73	2·59	2·88	1·13	2·40
12	21	19	12 5	12 4	2·21	2·39	2·88	2·58	1·52	2·10
11	19	26	11 6	11 5	2·79	2·35	2·58	2·62	1·63	2·27
10	24	31	10 6	10 5	1·96	2·13	2·60	2·45	2·04	2·42
9	12	24	9 6	9 7	1·25	1·83	2·54	2·69	1·75	2·71
8	7	23	8 5	8 5	0·93	1·43	2·64	2·39	1·57	2·74
7	20	18	7 6	7 5	0·23	0·31	2·10	1·92	1·50	1·78
6	4	6	6 10	6 10	0·13	0·33	1·86	2·58	1·50	2·16

Nearly all the girls of the 14-year old group have left the school, and some members of the higher age-groups have obtained scholarships to secondary schools or are attending central elementary schools, not so many as in the case of the boys' department, though, of those who are willing thus to extend their school education, the majority are Jewish. The 7-year old group is not quite complete, there are some 'sevens' in the infants' school. Only a very few of the abler 6-year old girls are, of course, to be found in the girls' school.

Making due allowances, or even without them, it is apparent that in the case of every age-group the balance of marks for reasoning is distinctly on the Jewish side. This is not the case with the marks for handwriting, where the balance of advantage shifts from side to side, neither Jews nor Christians, age for age, appearing to possess an advantage. In marks for social class the Jews have a distinct advantage in every case, and again it would seem that the whole superiority, where it exists, of the Jewish children may be the result of a social class factor and not of racial superiority. It is, perhaps, worth noting that, comparing the work of the boys with that of the girls, the boys, as usual, have the advantage in reasoning, and the girls have the advantage in handwriting, especially in the lower grades or standards. But the object of this research is not, except subsidiarily, a comparison of the work of boys and girls—we should require a guaranteed equality of pedagogical influences to be quite certain as to that issue—but rather the influence of a social class factor on the standard of school attainment of Christian and Jewish boys and girls, with some sidelights on that influence, which are much less securely based, of rational intelligence, so far as that is indicated by the results of two sets of reasoning tests, and on manual

ability, so far as that is indicated by the current handwriting of the children. Of the general importance of a 'social class' factor it is hardly necessary to speak, since now, even in official reports, it is so generally accepted that, before proceeding to a detailed account of the work done in the school, they frequently set out with what is really a social class description of the school, in the light of which either, favourably or unfavourably, the subjoined account must be read. But, though the influence of this 'social class' factor is now generally admitted, I have not yet shown in any detailed way the probable amount of its influence in the case of these Christian and Jewish boys and girls. To do this, I must give a numerical assessment for the school standards, and I have done so by allowing 7 marks for the seventh, the highest standard, 6 marks for the sixth standard, and so on, downwards. Then the correlation coefficient or r has been found between the social class mark, which, it will be remembered, ranges from 4 to 0, placing the children in five classes or groups. It has been worked out individually in all cases, omitting the 14-year old and the 6-year old children for obvious reasons. The numbers in some of the other cases are small (they will be found in Tables II and IV) and some allowance must be made for that.

Table V. *Showing for each age-group of boys and girls, Christian and Jewish children separately, the correlation coefficient between their school grading and their social class.*

Age-groups in years	Boys		Girls	
	Christians	Jews	Christians	Jews
13	+0·52	−0·11	+0·61	+0·33
12	+0·43	+0·38	+0·51	+0·67
11	+0·56	+0·08	+0·50	+0·19
10	+0·21	+0·21	+0·54	+0·23
9	−0·12	+0·15	−0·06	+0·69
8	+0·44	+0·34	+0·44	+0·51
7	+0·55	+0·26	+0·50	+0·16

There is, almost invariably, a fairly high positive correlation between the two series; such negative correlations as exist are very small, and, making allowance for all small coefficients, both positive and negative, we can accept the table as indicating clearly a positive relation. That social class is a causal factor has been elsewhere and otherwise demonstrated; this table only indicates, in a rough way, the extent to which it may, in this case, be relied on for each age-group of Jews and Christians. It is fairly certain that the influence of social class is considerable; and that it is on the side of the Jewish children, both boys and girls, we already know. It may account for, perhaps it may even more than account for, the superiority of the Jewish children, age for age. Is there

any residue of superiority due to race at all? If the children of the same age-group, and with the same social-class marks are shown in comparative tables, we ought to find an answer to that question. This I now proceed to set out.

Table VI. *Showing the children of the same marks for social class, and of the same age, compared in scholastic attainment, in reasoning and in handwriting.*

Boys.

	Jews.				Christians.			
Mark for social class	No. of boys	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing	No. of boys	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing
				13-year old group.				
4	1	7·00	4·00	3·00	2	6·00	2·75	3·75
3	8	5·88	2·50	2·25	4	7·00	3·50	2·88
2	7	6·43	2·79	2·43	4	6·25	2·75	2·50
1	1	7·00	3·50	1·50	5	5·60	2·80	2·70
0	1	6·00	2·50	2·00	1	5·00	3·00	3·00
				12-year old group.				
4	5	6·30	3·00	2·40	1	6·00	3·50	2·00
3	5	6·40	3·30	3·50	7	5·43	2·64	2·57
2	4	5·50	2·13	3·63	8	5·13	2·75	2·38
1	3	6·00	2·23	2·17	5	4·80	1·80	2·70
0	2	5·00	2·75	3·25	3	4·00	2·00	2·50
				11-year old group.				
4	6	5·50	2·33	2·83	0	—	—	—
3	7	5·29	2·57	3·21	2	4·50	1·50	3·00
2	3	5·00	2·33	2·50	6	5·00	2·00	2·92
1	5	5·40	2·80	3·40	4	3·75	1·38	2·25
0	1	5·00	3·00	2·50	4	3·25	1·25	2·25
				10-year old group.				
4	12	5·17	2·92	2·38	3	4·67	2·00	2·83
3	10	4·20	1·95	2·30	2	3·50	2·75	2·75
2	2	5·50	3·75	2·25	8	3·88	1·50	2·06
1	2	5·00	3·00	2·00	4	4·00	1·63	2·00
0	2	4·00	2·25	2·25	2	3·50	1·25	2·50
				9-year old group.				
4	6	4·00	2·08	2·50	1	4·00	3·50	3·00
3	5	4·00	1·70	2·40	3	3·00	1·17	2·00
2	6	3·83	2·00	1·92	2	3·00	1·25	1·25
1	1	3·00	0·50	1·50	1	4·00	2·50	1·50
0	1	4·00	3·00	1·50	2	3·50	0·75	1·50
				8-year old group.				
4	9	2·33	1·33	1·72	3	2·67	1·17	1·50
3	3	2·33	1·50	2·33	1	2·00	0·00	2·00
2	3	2·33	0·83	1·83	7	2·00	0·71	1·29
1	2	1·50	0·50	2·25	2	1·50	0·25	1·75
0	1	1·00	0·00	1·00	2	2·00	0·50	1·25
				7-year old group.				
4	4	1·75	0·75	1·88	1	2·00	1·50	2·00
3	7	1·57	0·36	1·64	1	1·00	0·00	1·00
2	4	1·00	0·00	0·63	6	1·17	0·08	1·00
1	1	1·00	0·00	1·50	6	1·00	0·00	1·00
0	1	2·00	0·00	0·50	1	1·00	0·00	0·00

Table VI (*contd*)

Girls.

Mark for social class	Jews. No. of girls	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing	Christians. No. of girls	Average mark for school grading	Average mark for reasoning	Average mark for hand-writing
				13-year old group.				
4	2	6·00	3·50	3·00	—	—	—	—
3	8	6·13	2·56	3·06	2	6·50	2·50	3·25
2	4	6·50	3·25	3·13	4	5·50	2·50	2·38
1	6	5·00	2·33	2·42	4	5·00	1·75	2·50
0	0	—	—	—	6	4·67	2·25	2·58
				12-year old group.				
4	2	6·00	2·50	2·50	—	—	—	—
3	4	6·00	2·50	2·88	1	7·00	2·00	4·00
2	9	5·78	2·28	2·61	10	5·30	2·45	2·85
1	2	5·50	2·50	2·25	9	4·67	2·17	2·67
0	2	4·00	2·50	2·25	1	4·00	0·50	4·00
				11-year old group.				
4	2	5·50	2·10	2·50	—	—	—	—
3	9	5·22	2·39	2·89	3	4·67	2·33	2·33
2	10	4·90	2·25	2·40	8	4·88	2·63	2·50
1	4	5·00	2·50	2·50	6	3·83	1·42	2·75
0	1	5·00	3·00	3·00	2	4·00	1·50	2·75
				10-year old group.				
4	5	4·75	1·80	2·70	2	6·00	3·50	2·50
3	9	4·56	2·67	2·61	6	4·50	2·33	2·67
2	13	4·08	1·81	2·23	10	4·00	1·55	2·25
1	2	4·00	3·25	2·25	3	3·33	1·83	3·17
0	2	4·00	1·50	2·75	3	4·00	1·67	2·83
				9-year old group.				
4	7	4·29	2·43	3·00	2	3·50	2·00	2·50
3	7	3·71	2·07	2·93	1	2·00	1·00	3·00
2	7	3·57	1·64	2·50	3	2·67	0·83	2·50
1	2	2·00	0·25	1·75	4	3·00	1·13	2·63
0	1	2·00	0·50	2·00	2	3·50	1·50	2·25
				8-year old group.				
4	5	3·40	1·60	2·10	0	—	—	—
3	9	2·67	1·61	2·50	1	3·00	2·50	2·50
2	7	2·57	1·29	2·29	2	2·00	0·00	3·25
1	2	2·00	0·75	3·00	4	2·25	1·00	2·38
0	0	—	—	—	0	—	—	—
				7-year old group.				
4	1	1·00	2·00	3·50	0	—	—	—
3	2	1·50	0·00	1·50	3	2·00	0·00	2·50
2	9	1·33	0·33	2·00	7	1·43	0·51	2·36
1	4	1·25	0·00	1·88	7	1·43	0·07	2·07
0	2	1·00	0·25	1·25	3	1·00	1·00	1·17

Comparing those of the same social class in each case, out of 34 possible comparisons, there are 24 cases in which the average school grading of the Jewish boys is superior to that of the Christian children, 3 in which it is the same, and 7 in which the Christian boys are superior.

Comparing the average marks for reasoning, there are 22 cases in which the Jewish boys are superior, 2 in which they are the same, and 10 in which the Christian children are superior. Comparing the average marks for handwriting, there are 16 cases in which the Jewish boys are superior, 3 in which they are the same as the Christian boys, and 15 in which the Christian boys are superior.

Turning now to the girls, out of 28 possible comparisons between children of the same social class and of the same age-groups, there are 13 cases in which the Jewish girls have superiority in school grading, 4 cases in which the average grading is the same, and 11 cases in which the Christian girls show superiority. In the average marks for reasoning, there are 17 cases in which the Jewish girls show superiority, 1 in which they are the same, and 10 in which the Christian children are superior. In handwriting there are 7 cases in which the Jewish girls are superior, 2 in which they are the same, and 19 in which the Christian girls are superior.

Summing up, it is difficult to resist the conclusion that Jewish children, both boys and girls, of the same age and social class as Christian children, are superior in their standard of school attainment, and, as far as our tests are indicative, the Jewish boys decidedly so in reasoning, the girls much more doubtfully, if at all, whilst in handwriting the superiority of the Jewish boys is very, very doubtful, and the Jewish girls are definitely inferior to the Christian girls. The differences in intelligence between Jews and Christians are greater in the male than in the female sex. The general contentions of the teachers are so far confirmed, namely, that Jewish boys and girls of elementary school age are more advanced for their ages and more intelligent, but so far as handwriting is indicative of manual dexterity, the Jewish boys appear about the same, and the Jewish girls definitely inferior to the Christian girls.

I have already given the correlation coefficient between the social class of the children and their school grading for each age-group of Christians and Jews separately. I now propose to show the r coefficient between their social class and the only intelligence tests which I have used, namely, the two sets of reasoning tests. And finally, I shall give the coefficients between the results of the reasoning tests and the results of the handwriting exercises, and I shall give the coefficients of variability (found by dividing the standard deviation by the average) of social class, school grading, reasoning and handwriting. These last comparative figures will enable us to check the current belief as to the greater variability of the Jewish than of the Christian children, which is so often asserted by teachers.

Table VII. *Showing the correlations (r coefficients) between social class and reasoning for each age-group of Jews and Christians, boys and girls separately.*

Age-groups in years	Boys		Girls	
	Christians	Jews	Christians	Jews
13	+0·21	+0·02	+0·20	+0·26
12	+0·37	+0·37	+0·36	+0·03
11	+0·24	−0·22	+0·52	−0·26
10	+0·30	+0·06	+0·44	+0·07
9	+0·42	+0·01	+0·15	+0·60
8	+0·32	+0·34	+0·26	+0·32
7	+0·65	+0·42	+0·23	+0·39

The coefficients are nearly all positive; they are generally lower in the case of the Jewish than of the Christian children: they are, on the whole, not so high as those between social class and school grading, which depends not on reasoning as such, but on an aggregate of marks for scholastic subjects, some only of which, as, for example, problematic arithmetic, involve reasoning.

Table VIII. *Showing the correlation coefficients between the results of the reasoning tests and the exercises in writing for Christians and Jews, for each age-group, boys and girls separately.*

Age-groups in years	Boys		Girls	
	Christians	Jews	Christians	Jews
13	+0·18	+0·03	+0·03	+0·32
12	−0·09	+0·04	−0·05	+0·17
11	+0·63	+0·28	−0·22	+0·13
10	+0·24	+0·18	+0·22	+0·17
9	+0·67	+0·28	+0·27	+0·24
8	−0·14	+0·15	+0·02	+0·19
7	+0·66	+0·37	+0·32	+0·58

The correlation coefficients are, as before, nearly all positive; but, with some outstanding exceptions, are decidedly low.

Table IX. *Showing the coefficients of variability in school grading, social class, reasoning and writing for each age-group of Christians and Jews.*

Age-groups in years	School grading				Social class			
	Boys		Girls		Boys		Girls	
	C.	J.	C.	J.	C.	J.	C.	J.
13	0·14	0·15	0·17	0·20	0·54	0·38	1·00	0·44
12	0·21	0·15	0·20	0·13	0·58	0·54	0·47	0·52
11	0·26	0·18	0·16	0·16	0·71	0·47	0·56	0·39
10	0·27	0·21	0·21	0·23	0·59	0·40	0·55	0·42
9	0·20	0·20	0·37	0·25	0·67	0·42	0·72	0·41
8	0·32	0·41	0·17	0·26	0·57	0·45	0·44	0·33
7	0·31	0·33	0·33	0·31	0·55	0·40	0·60	0·56

Table IX (*contd*).

Age-groups in years	Reasoning Boys C.	Reasoning Boys J.	Reasoning Girls C.	Reasoning Girls J.	Writing Boys C.	Writing Boys J.	Writing Girls C.	Writing Girls J.
13	0·18	0·32	0·27	0·30	0·25	0·30	0·23	0·24
12	0·38	0·25	0·32	0·17	0·32	0·23	0·21	0·19
11	0·56	0·31	0·38	0·22	0·35	0·27	0·15	0·23
10	0·53	0·39	0·45	0·43	0·35	0·39	0·23	0·28
9	0·67	0·58	0·77	0·56	0·39	0·27	0·20	0·26
8	*1·17	1·00	1·11	0·50	0·43	0·32	0·23	0·25
7	*4·00	2·00	2·15	2·00	0·70	0·50	0·29	0·42

* The high variability in reasoning in the lower age-groups *may* be an indication that the questions are not graded closely enough, and are not adequate tests of reasoning at these ages.

It is a conservative conclusion that, disregarding the few children who have obtained scholarships or are attending Central schools, of which more than a fair proportion are Jewish, and disregarding the very few children who have been accepted in schools for the mentally defective, and have been so allocated at the statutory examinations therefore, and considering the children remaining in the ordinary elementary school, the variability of the Jews is slightly less than that of the Christians in school grading, is less in social class and in reasoning, and is less in writing on the whole, as far as the boys are concerned, and more in writing as far as the girls are concerned. There is apparently no justification for the ordinary opinion that Jewish children are more variable intellectually.

VII. SUMMARIZED CONCLUSIONS.

1. That Jewish children attending East-end Elementary Schools are superior intellectually to Christian children.

2. That much of this superiority is due, not to race, but to a superiority of social class.

3. That, when children of the same social class are compared, the Jewish children still show some intellectual superiority.

4. That the difference between the Jewish boys and Christian boys is greater than that between the Jewish girls and Christian girls.

5. That in manual dexterity, so far as is shown by handwriting, Jewish and Christian boys are more nearly alike, and Christian girls are superior to Jewish girls.

6. That the variabilities within the same age-groups of the Jews are somewhat less marked than those of the Christians, except in the case of the writing of the Jewish girls.

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THE MEASUREMENT OF INTELLIGENCE IN A RURAL AREA.

By J. B. RUSSELL.

I. Introduction.

In 1920–1 Godfrey H. Thomson prepared a Group Mental Test for use in Northumberland, and the results of his researches were published in this *Journal*[1].

In 1925 the same test was used, with the concurrence of the Local Education Authorities, to effect a mental survey of two age groups in an Eastern Administrative County and the central Borough.

In the interval, 1925–8, some further information has been obtained by a study of:

(*a*) the abilities of children whose parents follow agricultural occupations;

[1] This *Journal*, 1921, XII (3), and 1923, XIV (2).

(*b*) the ancestry of a selected group;
(*c*) the replies of teachers to questionnaires sent out in 1927, two years after the date of the test.

II. Methods.

Object of the test.

The primary object of the test was to make a first selection of gifted children in attendance at elementary schools in the areas concerned.

It was anticipated also that the mental test might reveal at least one of the reasons why many elementary schools rarely submit candidates for the examination in English and Arithmetic, on the result of which free places are allotted in secondary schools.

For geographical reasons, it was obvious that the tests would have to be applied in the elementary school under the supervision of the teachers. To secure uniformity of treatment for all examinees, it appeared desirable that little or nothing should be left to the discretion of supervisors, that the actual working of the test should not take more than about an hour, and that the marking of the papers under supervision should be expeditious.

Northumberland test, No. 1, appeared in every way to meet these requirements and its final selection was not unconnected with the circumstance that between 1923–5 I had used the test with increasing confidence for purposes of diagnosis in individual cases.

Preliminary arrangements.

The authorities sent two forms to each school, on one of which head teachers entered the following information in columns:

1. Name:
2. Date of birth:
3. Age in years and months on March 31st, 1925:
4. Head teacher's estimate of ability on a five-point scale, A–E:
5. Class on March 31st, 1925, before promotion.

On the same form, columns headed 'Mental Age,' 'Age,' 'I.Q.' were left vacant for the use of assistant examiners.

On the day preceding the test proper, a 10 minutes' preliminary practice test was worked.

On April 2nd, 1925, all children of 10 and 11 years of age in the

county and the borough worked through Northumberland test, No. 1, between 10 and 11 a.m., and the papers were returned to head-quarters on the same day.

The time allowance was found to be ample, and nearly all the children completed as much as they were able to do within the limited period.

In all, 3379 children, distributed as follows, worked through the test:

	County	Borough
Boys, aged 10	770	111
Girls, ,, 10	745	116
Boys, ,, 11	728	92
Girls, ,, 11	715	102
	2958	421 = 3379

Marking and tabulation.

The worked booklets were marked in one room, under my supervision, by a team of teachers selected from the elementary and secondary schools, to all of whom I am greatly indebted[1].

For transmission to the schools, the I.Q. marks for each examinee were entered on a record form on which the names had previously been written. The form provided a column for the father's occupation as well as spaces for the entry of the names of those children, who although they did not reach the score of I.Q. 115 +, fixed as the minimum mark for the first selection of candidates, were specially recommended (*a*) by head teachers, or (*b*) desired by their parents to sit for the subsequent attainment examination in English and Arithmetic.

In the majority of cases, no names were entered under (*a*).

In Table I the massed results of the mental test are expressed as percentages in terms of Intelligence Quotients, and in Tables II A and II B further analyses of the scores are made.

A comparison of columns (1) and (2) Table II A shows that a higher percentage of girls than of boys scored 120 +, and this is in accord with the other data for the county area.

The somewhat lower scores in column (4) are doubtless due to the early transfer of some children to secondary schools, but on the whole there is close agreement for the brighter children (columns (3) and (4)).

[1] To Mr W. J. Bright, Mr W. A. Chatters, Mr E. G. Davis, Mr S. H. Fitch, Mr A. L. Hurst, Mr H. H. Sterne and Mrs G. E. Sizer, I am specially indebted for the help they have given me during the period 1925–8.

Table I. *Percentages.*

	An Eastern County		Borough	
I.Q.'s	No. of children	%	No. of children	%
Above 140	7	0·2	4	0·95
131–140	37	1·3	3	0·71
121–130	99	3·3	24	5·7
111–120	301	10·2	51	12·12
101–110	560	18·9	97	23·04
91–100	793	26·8	126	29·91
81– 90	568	19·2	73	17·34
Below 81	593	20·1	43	10·2

Table II A. *County area.*

I.Q.'s expressed as percentages. Separation of the 10 and 11 age groups.

	10 years old			11 years old
	Boys (1)	Girls (2)	Boys and girls (3)	Boys and girls (4)
Above 140	0·4	0·4	0·4	0·1
131–140	1·2	1·0	1·1	1·3
121–130	2·5	4·4	3·3	3·3
111–120	9·1	12·4	10·6	9·7
101–110	18·4	22·7	20·5	17·4
91–100	24·3	27·6	25·9	27·7
81– 90	19·9	17·9	19·0	19·4
Below 81	24·2	13·6	18·2	21·1

Table II B. *Borough area.*

I.Q.'s expressed as percentages. Separation of the 10 and 11 age groups.

	10 years old			11 years old
	Boys	Girls	Boys and girls	Boys and girls
Above 140	1·8	1·7	1·9	—
131–140	0·9	—	0·5	0·9
121–130	9·0	6·8	7·9	3·0
111–120	15·3	11·3	13·2	11·1
101–110	23·5	19·9	21·6	24·9
91–100	20·7	32·8	26·8	33·6
81– 90	17·1	18·2	17·7	16·8
Below 81	11·7	9·3	10·5	9·7

In Table II B there is a marked difference between boys and girls aged 10—a difference that has been found in a subsequent investigation for a group 10–13 years of age.

The main differences between columns (3) and (4) are again due to the transfer of children to secondary schools.

III. Features of the survey.

The outstanding features of the county survey are that 4·8 per cent. of the children of 10 and 11 years of age scored I.Q.'s of 121 + and that 10·3 per cent. scored 115 +.

Prof. Thomson informs me that on the average he finds that 5 per cent.[1] of English children score 120 + and thus the figure 4·8 for this county does not differ significantly from his results in other areas.

On the other hand, there is a long tail, amounting to 20 per cent. with I.Q. scores below 81, and 28 per cent. of the departments produced no child with an I.Q. of 115 +.

There is a great difference between the county and the borough results, for in the borough 7·3 per cent. scored 121 + and 10·2 per cent. below 81.

Comparison with Northumberland.

On February 24th, 1922, all elementary school children in the county of Northumberland who were over 11 years and under 13 years of age worked a mental test and from data kindly communicated to me by G. H. Thomson it has been possible to estimate the I.Q. distribution of the Northumberland 11-year-old children for comparison with children of the same age in the present investigation.

Table III. *Percentages.*

	I.Q.							
	Above 140	131–140	121–130	111–120	101–110	91–100	81–90	Below 81
Northumberland	0·2	1·5	5·4	17·8	26·8	24·0	17·0	7·3
An Eastern county	0·1	1·3	3·3	9·7	17·4	27·7	19·4	21·1

The physical and occupational characteristics of the people are very different in the two counties, and Table III reveals a marked divergence in the mentality of the children.

Contrasting the two counties, it is to be observed that while the proportion of rural acreage in the two counties does not differ by more than 1 per cent., 62 per cent. of the population of my own county is rural compared with 14 per cent. of Northumberland.

The following figures, taken from the Registrar-General's Census, 1921, give the population per 1000 males, age 12 and over, engaged in the six industries employing the greatest number of men in the administrative counties.

[1] He has slowly come to the conclusion that the percentage is rather more than 5.

An Eastern county		Northumberland	
1. Agricultural occupations ...	365	1. Mining and quarrying ...	202
2. Commerce	63	2. Metal workers	147
3. Undefined workers (mainly labourers)...	44	3. Transport	90
4. Builders	43	4. Commerce	65
5. Metal workers	43	5. Undefined workers (mainly labourers)...	64
6. Road transport workers ...	35	6. Agricultural occupations ...	53

The urban area of my county occupies only 6 per cent. of the county and the largest town has a population of 15,937, situated in the centre of 600 square miles of country. The Registrar-General reports that the county "with its general population of 365 agricultural workers per 1000, and its rural population of 521, is the most exclusively agricultural county so far reported on."

In rural Northumberland, the largest group of men, 198 per 1000, is engaged in coal mining, and the population is four times as dense as in the rural parts of the Eastern county, but in the latter county there are three times as many agricultural labourers.

It is now possible to compare the abilities of the children whose fathers are engaged in the main occupational groups characteristic of Northumberland and my own county.

Duff and Thomson[1] have shown that the average I.Q.'s of the children of miners, quarrymen and metal workers were as follows:

Miners (colliery workers miscellaneous)	98·4
Quarrymen	98·6
Metal workers	100·9

The scores of 595 children whose fathers follow agricultural occupations are classified in Table IV.

For the purposes of this investigation, rural schools were chosen likely to have many children of farm workers. The agricultural occupations were recorded of the fathers of all children who took the mental test in 1925.

IV. The abilities of children whose fathers follow agricultural occupations.

The figures in column (3) give more accurate information than the lower limits indicated in column (2), for the Northumberland test, No. 1, is not designed to measure I.Q.'s below 80. The lower limits in column (2) are estimates only arrived at by an extension of the table given on p. 5 of Thomson's *Manual of Directions* and by a consideration of the class position of each child.

[1] Duff and Thomson, this *Journal*, 1923, XIV (2), 194.

Table IV.

	(1) No. of children	(2) Range of I.Q.	(3) No. scoring I.Q. 80 −	(4) No. scoring I.Q. 120 +	(5) Median	(6) I.Q. for small groups
Gardener	24	126–68	2	1	104	—
Foreman on farm	17	147–83	0	2	99	—
Skilled artizan*	146	149–64	21 (14)	17 (11)	98	—
Blacksmith	17	126–73	2	2	97	—
Farmer	43	132–68	3 (7)	2 (4)	91	—
Small-holder	25	121–67	7 (28)	1 (4)	91	—
Cowman	14	115–71	3	0	91	—
Horseman	72	121–64	15 (20)	1 (1)	90	—
Shepherd	9	103–67	3	0	90	—
Stockman	11	124–72	1	1	88	—
Farm labourer	187	120–60	60 (32)	2 (1)	87	—
Gamekeeper	10	122–69	4	1	82	—
Motor ploughman	4	—	—	—	—	113, 94, 90, 81
Dairyman	3	—	—	—	—	106, 102, 76
Woodman	6	—	—	—	—	122, 116, 108, 107, 93, 92
Malting labourer	7	—	—	—	—	122, 109, 107, 106, 99, 90, 87

The figures in brackets represent percentages.
* Included for comparison.

For Northumberland test, No. 1, Thomson[1] found a correlation of about 0·8 with Terman's Stanford Revision of the Binet Scale.

In an independent calculation undertaken to check the accuracy of the estimates mentioned above, the correlation worked out at 0·8 + for a complete group of 26 children, including some sub-normal children, in one school.

Other figures for sub-normal children are given in columns *A* and *B*, and indicate the degree of approximation for the lower limits in column (2) of Table IV.

	A Estimated as described	*B* Terman's Stanford Revision of the Binet Scale
A	77	74
B	64	62
C	61	54
D	65	68
E	66	71
F	67	73
G	71	74

Table IV shows that the median for farm labourers is rather lower than that for cowmen, horsemen or shepherds, the *élite* of the workers on farms. Of the children of farm labourers, 1 per cent. scored I.Q. 120 + and 32 per cent. scored less than I.Q. 80.

[1] Duff and Thomson, this *Journal*, 1923, XIV (2), 194.

For agricultural workers of all classes in Northumberland, the average I.Q. worked out at 97·6—a figure considerably above the indications of Table IV. Moreover, the average I.Q.'s for the children of miners, quarrymen and metal workers are higher than for agricultural workers in either area. In this connection it is important to note that "Miners and agricultural labourers have approximately the same effective fertility, though they reach this result by different paths. The former have a high birth rate and a high child mortality and the latter a lower birth rate and a low child mortality[1]."

Thus the marked differences observable in Table III are not unassociated with the general characteristics of the population in the two counties. Moreover, the percentage of mentally defective children attending public elementary schools is more than twice as great in the Eastern county as in Northumberland[2].

Although the number of cases summarized in Table IV is small compared with the extensive research of Haggerty and Nash[3] in the United States of America and the still wider enquiry of Duff and Thomson in England, the degree of agreement, fortuitous though it may be, appears to be noteworthy.

Table V.

	Average I.Q.	Medians	
	Duff and Thomson	Haggerty and Nash	This paper
Skilled artizans	102 (approx.)	98	98
Blacksmiths	101·4	95	97
Farmers	98·4	91	91
Unskilled labourers	94·3	89	87
	(unclassified)		(farm labourers)

A comparison of rural schools.

In a recent paper[1], D. Caradoc Jones and A. M. Carr-Saunders gave a useful bibliography of the work done on "The Relation between Intelligence and Social Status," and they refer specifically to investigators, who (*a*) classify children according as to whether they live in 'good' or 'bad' parts of the town, and (*b*) those which classify them according to the occupations of the fathers.

The borough results under (*a*) are in accord with previous work, and although marked differences in the general level of intelligence were

[1] Carr-Saunders and Caradoc Jones, *Social Structure of England and Wales*, Oxford 1927.

[2] Annual School Report, 1926.

[3] *Journal of Educational Psychology*, 1924, xv.

known to exist in rural schools, the magnitude of the variations, in an area almost exclusively devoted to agriculture, is greater than anticipated.

In Table VI schools of approximately the same size are compared, and in column (8) the distances from the nearest railway station (R.) and town (T.) are given.

Table VI.

(1) Index No.	(2) No. of children in school	(3) No. of children tested	(4) % of col. (3) scoring under 100 I.Q.	(5) % of col. (3) scoring over 100 I.Q.	(6) I.Q.'s 115 and over	(7) Average I.Q.	(8) Miles from nearest railway (R.) R.	town (T.) T.
Schools with less than 50 children:								
138	46	11	90·9	9·1	Nil	86·4	3	7½
134	45	15	80·0	20·0	Nil	85·6	½	4
146	37	11	63·6	36·4	116, 116, 127, 133	99·3	4	8
112	32	13	53·8	46·2	Nil	93·1	4	4
151	39	11	27·3	72·7	117, 120, 120	105·1	8	7
72	33	11	27·3	72·7	116, 117	105·2	5	6
Schools with 50 to 100 children:								
62	84	26	84·6	15·4	115	86·7	4	4
114	80	21	80·9	19·1	Nil	82·6	4½	6
32	68	20	80·0	20·0	117	90·0	4	7
79	97	23	73·9	26·1	116, 116	90·0	5	7
111	91	23	65·2	34·8	116, 119, 147	88·0	2	2
70	84	21	52·4	47·6	115, 116, 120	99·0	4½	9
Schools with 100 to 150 children:								
126	104	31	80·6	19·4	115, 116	92·4	1½	5
149 (B.)	144	45	75·6	24·4	128, 133, 135	88·2	0	T.
8	109	38	52·6	47·4	117, 122, 124, 134	97·4	6	8½
26	134	33	42·4	57·6	115, 115, 115, 124, 130	102·3	0	5½
94	145	40	35·0	65·0	115, 115, 116, 116, 119, 119, 119, 120, 120, 124, 125, 125, 126, 132, 140	106·8	0	7
121 (B.)	126	43	30·2	69·8	115, 131	92·6	0	T.
Schools with over 150 children:								
71 (B.)	208	71	80·3	19·7	121	86·8	Town with railway station	
118 (M.)	338	125	71·2	28·8	115, 115, 115, 116, 116, 118, 120, 120, 122, 136, 142	90·7	,,	
71 (M.)	249	67	61·2	38·8	117, 118, 121, 123, 133	93·0	,,	
148 (M.)	311	90	56·7	43·3	115, 116, 117, 118, 121, 122, 123, 125, 130, 138	98·1	,,	
121 (M.)	260	45	51·1	48·9	119, 119, 120, 120, 122, 123, 123, 126, 138, 140	102·7	,,	
109	154	48	20·8	79·2	115, 115, 116, 117, 118, 118, 120, 120, 127, 127, 129, 129, 132, 134	107·5	,,	

V. A comparison of teachers' estimates with the result of the mental test.

It has already been stated that before the day fixed for the mental test, teachers were asked to classify the children into five groups:

A. To mean exceptionally intelligent.
B. To mean a little above the average.
C. To mean average ability.
D. To mean a little below the average.
E. To mean exceptionally dull.

The massed results for the county area are given in Table VII.

Table VII. *Teachers' estimates expressed as percentages.*

Ages of children (boys and girls)	A	B	C	D	E
10	2·4	10·9	40·6	31·0	14·9
11	2·0	10·0	45·0	30·0	13·0
10 and 11	2·2	10·5	42·8	30·5	14·0
Total	12·7 Advanced		42·8 Normal	44·5 Retarded	

For comparison with the teachers' estimates, a table was constructed expressing the difference between the mental age as determined by the test, and the chronological age of each child.

Table VIII shows the percentage of normal children, *i.e.* children whose mental age is neither advanced nor retarded by 1 year; and the percentages of children whose mental age is advanced or retarded by 1 to 4 years. Children retarded by more than 4 years are included in the − 4 group.

Table VIII. *Mental test.*

Advancement and retardation expressed as percentages.

Ages of children (boys and girls)	Advanced +4 y.	+3 y.	+2 y.	+1 y.	Normal	Retarded −1 y.	−2 y.	−3 y.	−4 y.
10	0·3	1·6	4·5	11·4	43·0	18·4	10·7	9·4	0·4
11	0·9	1·2	4·3	10·6	39·4	17·9	13·7	6·9	4·8
10 and 11	0·6	1·4	4·4	11·0	41·2	18·1	12·2	8·1	2·6
Total	17·4 Advanced				41·2 Normal	41 Retarded			

The comparison may therefore be expressed thus:

	Mental test %	Teachers' estimates %
Children advanced	17·4	12·7
Normal children	41·2	42·8
Children retarded	41·0	44·5

For the normal children, the figures agree fairly well, but for the brighter children, there is a considerable discrepancy due to the fact that the method of calculating I.Q.'s makes a proper allowance for differences in age, while there is no such automatic check underlying the teachers' estimates. Thus, it not infrequently happened that younger children who scored as much as 120 + were marked C. For example, in the borough, where 31 children scored 120 +, 13 were marked C.

A scrutiny of the returns shows that the standard of individual teachers varies, and depends on the quality of the children in the particular school. In schools where the general level of ability is low, a child of barely average ability may be marked B or even A and *vice versa*.

R. E. Marsden in a personal note dealing with an investigation in the S.W. of England confirms this view. He says: "In these small schools, teachers know the relative capacities of the children in their own schools, but they are not so well aware of the quality of their children as compared with those in other schools. The teacher's average child varies with the quality of the children in the school."

A re-survey after two years.

Notwithstanding the difficulties indicated in the previous section, it appeared possible that interesting information might be obtained through the medium of a questionnaire addressed to teachers before these children ceased to attend school. By the courtesy of the editor of the local Teachers' News, information was asked for, and replies were received from 59 schools.

1st Questionnaire	Analysis of replies
1. No. of children who took the mental test in 1925?	1. 1588.
2. No. of children who scored 95–105?	2. 506.
3. Is your present estimate of the abilities of these children in general agreement with the result of the mental test?	3. Seven head teachers say they disagree with the general result of the test for the 95–105 group. On analysis of the return the total number of children concerned in the seven schools is eleven.
4. In how many cases do you disagree?	4. There is disagreement in 77 out of 506 cases, *i.e.* 15 per cent.
5. In how many instances has the mental test led you to revise your opinion of individual children?	5. The test led to a revised opinion in 58 cases, 3·6 per cent.
6. How many children who scored 115 and over have been given accelerated promotion?	6. 94 children.
7. Have you found that promotion in these cases was justified?	7. Yes, except in 4 cases.

A second and fuller questionnaire was sent to another group of schools and 88 replies were received.

2nd Questionnaire	Analysis of replies
1. No. of children who took the test in 1925?	1. 1701.
2. No. of children who scored 95–105 inclusive? Percentage?	2. In schools with 20 or more examinees, the percentage varies from 9·5 to 63.
3. Is your present estimate of the abilities of this 95–105 group in general agreement with the result of the mental test?	3. 90 per cent. of the head teachers agree with the general result. Nine head teachers are not in agreement concerning a total of 21 children.
4. In how many cases in this group (95–105) do you disagree?	4. There is disagreement in 54 out of 523 cases, 10 per cent.
5. Is your present estimate of the abilities of the children who scored less than 95, or over 105, in general agreement with the result of the mental test?	5. 88 per cent. of the head teachers agree with the general result. Eleven head teachers are not in agreement concerning a total of 30 children.
6. In how many of the last-mentioned cases do you disagree?	6. There is disagreement in 109 out of 1178 cases, 9 per cent.
7. In how many cases has the result of the test caused you to revise your estimate of the ability of the children?	7. In 47 cases, 4 per cent.
8. How many children who scored 115 and over have been given accelerated promotion?	8. 93.
9. Have you found the promotion in these cases justified?	9. Yes, except in 2 cases.

The most interesting fact that emerges from this enquiry is that of 187 children who scored I.Q. 115 + in 1925 and were given accelerated promotion in the elementary schools in which they remained, all but 6 are reported in 1927 to be doing well, and head teachers state that the accelerated promotion of these children was justified.

The remaining replies to the questionnaire based on marks or subjective estimates may be compared with the results of a public examination and estimates of a similar character.

The heads of five secondary schools in the same area were asked to state:

(*a*) The number of cases in which the result of the School Certificate Examination amounted to a complete surprise.

(*b*) Number of cases in which minor variations from the Secondary School estimates occurred.

For this examination, 59 candidates sat. Two cases were reported under (*a*) and six under (*b*), a variation of 13 per cent.

Relationship of the mental test to a standardized attainment examination.

Much work has been done on the relationship of Intelligence Tests to normal examinations, for example R. R. Dobson[1] has shown that the I.Q.'s of various groups of boys and girls at school correlate with school estimates from 0·62 to 0·78.

[1] Dobson, this *Journal*, XV.

Again, Wood[1] found a correlation of 0·70 between college success and the Thorndike Intelligence Test.

It is now possible to compare the results of the mental test carried out in the Eastern County with the results of a standardized attainment examination in Arithmetic and English.

For this examination, Dr Ballard's standardized papers in Arithmetic (Problems) and English (Silent Reading)[2], were used and a table was constructed from which the marks scored could be read in terms of 'subject' ages for English and Arithmetic. By adopting the method of Educational Quotients[3], a satisfactory allowance for age was made and the results were calculated thus:

$$\text{Educational Quotient (E.Q.)} = \frac{\text{Educational age}}{\text{Chronological age}} \times 100,$$

where the term educational age stands for the mean of the 'subject' ages for the two papers of the examination.

The degree of correspondence between the results of the mental test and the results of the attainment examination for a group of children at a secondary school and for the children of 10 and 11 in attendance at elementary schools in two small towns, A and B, is indicated:

(*a*) by writing down, for the secondary school group, the orders of merit (*i.e.* the I.Q. and E.Q. orders) in the two examinations;

(*b*) by expressing the degree of correspondence in terms of coefficients of correlation calculated by using Spearman's formula for ranks with Pearson's correction.

Secondary school group.

Mental test (I.Q. order)	1	2	3	4	5	6	7	9	9	9	11	12½	12½	14½	14½	16
English and arithmetic (E.Q. order)	1	2	3	4	5½*	7½	10½	5½	7½	9	10½	15	16	12	13	14

* The usual practice has been adopted. The 'ties' for the 5th rank are given the rank 5½.

Here, the children, 1st, 2nd, 3rd and 4th, in the mental test were 1st, 2nd, 3rd and 4th respectively in the attainment examination and the 16th in the mental test was 14th in the attainment examination.

Children in towns A and B and the secondary school group. The majority of children at A and B were pupils who scored I.Q. 115 +. At A there were 12, and at B 17 children, who scored between 85–114 I.Q. and were either selected by their head teachers or desired by their parents to sit for the attainment examination.

[1] Wood, *Measurement in Higher Education, Columbia University.*

[2] Dr P. H. Ballard, *The New Examiner*, 1923, pp. 163–99.

[3] In this connection, I am indebted to C. A. Richardson for helpful criticism.

Table IX.

	No. of pupils	Age* range	Range of I.Q.	Range of E.Q.	Coefficients of correlation	P.E.
Town A	41	10·3–12·1	85–142	70–155	0·81	+0·038
Town B	42	10·2–12·1	85–147	85–144	0·80	+0·038
Sec. school	16	10·0–12·5	99–144	83–147	—	—

* Years and months.

VI. The ancestry of a selected group of superior children.

Vocations of parents and grandparents.

By the researches of Duff and Thomson, Haggerty and Nash, and other investigators, it has been established that on the average children of 'lower status' do less well in intelligence tests than do children of 'higher status,' and it has also been shown that it is not possible to predict from occupation to I.Q. in individual cases.

The wider question how far does a person's ability tend to correspond with that of his immediate ancestors as measured by status brings in the mother and the grandparents.

In Table X the vocations of parents and grandparents are given for a selected group of children, all of whom scored 130 + in Northumberland test, No. 1.

The Roman figures refer as a rule to the classification of vocations prepared by Winifred Spielman and C. Burt[1]. Doubtful figures are enclosed in brackets.

Class I. Higher professional administrative work. Mental ratio over 150.

Class II. Lower professional, technical and executive work. Mental ratio 130–150.

Class III. Clerical and highly skilled work. Mental ratio 115–130.

Class IV. Skilled work. Mental ratio 100–115.

Class V. Semi-skilled repetition work. Mental ratio 85–100.

Class VI. Unskilled repetition work. Mental ratio 70–85.

Class VII. Casual labour. Mental ratio 50–70.

Class VIII. Institutional. Mental ratio under 50.

[1] *A Study in Vocational Guidance*, Table IV, p. 16, Industrial Fatigue Research Board.

Table X.

						Father's side		Mother's side	
Ref. No.	Index No.	Sex	I.Q.	Father	Mother	Grandfather	Grandmother	Grandfather	Grandmo
C.	19	G.	153	Blacksmith IV	Teacher II	Blacksmith IV	—	Farmer IV	D.S. V or V
B.	2	G.	153	Engineer's foreman III	Cook V	Bricklayer V	Dressmaker IV	Brickmaker V	Housekee
B.	1	B.	150	Jeweller's porter V	D.S. V or VI	Porter V	—	Shepherd VI	D.S. V or V
C.	113	B.	144	Carpenter IV	D.S. V or VI	Carpenter IV	—	Dockmaster III	Home V or V
C.	57	G.	143	Postman V	D.S. V or VI	Horseman VI	D.S. V or VI	Blacksmith IV	D.S. V or V
C. (N.)	118	B.	142	Compositor III	Teacher (elem.) II	Jockey	D.S. V or VI	Turf correspondent III	D.S. V or V
B.	3	B.	141	Soldier	D.S. V or VI	Drayman V	D.S. V or VI	Horsekeeper VI	D.S. V or V
B.	3	B.	140	Postman V	Nursery governess III	Head postman III	Cook V	Accountant's clerk III	Court dressmak III
C. (N.)	121	G.	140	Grocer's clerk IV	Parlour maid V	Varied	Dressmaker IV	Gardener	D.S. V or V
C.	58	B.	139	Small-holder	D.S. V or VI	Farm labourer VI	D.S. V or VI	Engine driver IV	Factory h VI
C. (S.)	148	B.	138	Sergeant (musketry instructor)	Silk warper V	Mat maker VI	D.S. V or VI	Mat maker VI	D.S. V or V
C.	57	G.	137	Station agent (Canada) III	Teacher II	Factory manager II	Home	Police constable IV	D.S. V or V
C.	65	B.	136	Stockman VI	D.S. V or VI	Farm labourer VI	D.S. V or VI	Gardener	Machinist, cloth fact V
C. (H.)	68	G.	136	Machinist V	Dressmaker IV	Farm worker VI	Housekeeper	Farm worker VI	Dairy mai
C. (N.)	119	B.	136	Carpenter IV	Home	Carpenter IV	—	Farmer V	—
C.	109	B.	134	Corn merchant III	Home	General shop-keeper III	—	Clerk III	—
C.	146	B.	133	Horseman VI	D.S. V or VI	Horseman VI	D.S. V or VI	Shepherd VI	D.S. V or V
C.	100	G.	133	Groom VI	D.S. V or VI	Groom VI	Nurse III	Brickmaker V	Cook V
C. (H.)	71	G.	133	Engineer's labourer VI	Machinist V	Engineer's labourer VI	Machinist V	Weaver V	Factory finisher V
C. (N.)	106	B.	131	Mechanical engineer II	Teacher (elem.) II	Engineer II	Home	Sailor	Cook (V)
C.	106	G.	131	Drug worker VI	Cook V	Drug worker VI	Dressmaker IV	Weaver V	Weaver V

Note. D.S. = domestic servant. Column 1, Schools: B. = borough; C. (S.), C. (N.), C. (H.) = small towns; C. = villag

Index No.	Sex	I.Q.	Father	Mother	Father's side: Grandfather	Father's side: Grandmother	Mother's side: Grandfather	Mother's side: Grandmother
106	B.	131	Malting labourer VI	Home	Labourer VI	Home	Labourer VI	Home
119	G.	130	School teacher II	Home IV	Business IV	—	Engineer III	—
26	B.	130	Painter V	D.S. V or VI	Farm worker VI	D.S. V or VI	Farm worker VI	D.S. V or VI
148	B.	130	Master baker (IV)	Shop assistant IV	Carpenter V	Housemaid V	Jeweller III	Ladies' hairdresser IV
2	G.	130	Grocer III	Housekeeper III	Labourer VI	Maid V	Labourer VI	Dressmaker IV

Note. D.S.=domestic servant. Column 1, School: B.=borough; C. (S), C. (N.), C. (H.)=small towns; C.=village.

Two problems appeared worthy of investigation:

(1) Does information concerning the status of both parents and the grandparents afford evidence of greater prognostic value than data with respect to the father alone?

(2) In a rural area is there any relationship between the I.Q.'s scored by children of the selected group and the birthplaces of the immediate ancestors?

The results obtained from (2) led me to record the birthplaces of the parents of a group of children who scored I.Q.'s of less than 80.

(1) Looking at Table X, the general character of the parental and grandparental ancestry is sufficiently obvious. An inspection of the data in each horizontal line indicates that 12 children had at least one relative of superior status, but for convenience of description reference is made below to the classification previously described. Of the 156 ancestors, 25 only (16 per cent.) had occupations in Class III or higher.

Of the 26 children, 12 had a parent or grandparent in Group III or a higher group, and in 5 of the 12 cases three of the ancestors may be so classified.

On further analysis of this incomplete table the ancestors of superior status are distributed as follows:

3 cases	Mother	Father	1 grandparent
1 case	—	Father	2 grandparents
1 ,,	—	Father	1 grandparent
1 ,,	Mother	—	3 grandparents
1 ,,	Mother	Father	—
1 ,,	—	Father only	—
1 ,,	Mother only	—	—
3 cases	—	—	A grandparent only

Fourteen children had no ancestor in Groups I to III.

It seems, therefore, that the answer to the first question is that a record of the status of the ancestors is of greater prognostic value than that of the father alone, but in more than 50 per cent. of the cases no forecast from status to I.Q. is possible.

The tentative hypothesis advanced in the 'Discussion' may account for this circumstance in the selected group[1].

(2) The relationship of I.Q.'s to the birthplaces of the immediate ancestors. When parents or grandparents are born in the same village, is an environmental effect—arising from a restricted outlook and its influence on the children—to be noted?

The group chosen for investigation included all who were awarded free places in 1925. The birthplaces of some parents could not be obtained, but as it appeared desirable to make the list as long as possible three children who scored I.Q.'s of 115 +, and for whose parents birthplaces were known, were included.

Table XI shows the birthplaces of each parent and grandparent and the number of ancestors born in the same village or town.

Parents or grandparents born in widely different counties (at least 6 counties distant) are indicated in the table	W.D.C.
Parents or grandparents born in different counties	D.C.
Parents or grandparents born in the same county	S.C.
Parents or grandparents born in different villages of the same county	D.V.
Parents or grandparents born in the same village	S.V.

S.V. in the case of grandparents refers to them alone, but the data in column (7) supplements this information.

When the cases now given in Tables XI, XII and XIII were arranged in descending order of merit, and the list divided into two equal sections—(1) with higher I.Q.'s, and (2) with lower values—it became evident that the birthplaces of the ancestors of (1) were slightly more static than the birthplaces of (2), so that no advantage due to varied environment appeared.

It was then decided to dissect the table into three parts to separate the parents born (*a*) in villages, from parents born in (*b*) local towns, (*c*) distant towns. The discovery was then made that in all but two cases of group (*a*) Table XI the parents were born in different villages or counties, and when grandparents were included the ratio of S.V. in the first half of the table to S.V. in the second half was as 9 : 14.

[1] Karl Pearson, "The Law of Ancestral Heredity," *Biometrika*, 1902–3, II, 211.

[2] Karl Pearson, "On the inheritance of the Mental and Moral Characters in Man," *Biometrika*, 1904, IV (2), 131.

Birthplaces of immediate ancestors.

Table XI. *Villages.*

(1) Ref. No.	(2) Sex	(3) I.Q.	(4) Father and mother W.D.C.	D.C.	S.C.	D.V.	S.V.	(5) Father's side (grandparents) W.D.C.	D.C.	S.C.	D.V.	S.V.	(6) Mother's side (grandparents) W.D.C.	D.C.	S.C.	D.V.	S.V.	(7) No. of ancestors born in one village
3	G.	153	.	.	×	.	×	.	.	×	×	.	.	.	×	.	×	4
113	B.	144	×	. .	.	.	.	.	.	×	.	×	.	.	×	.	×	3
57	G.	143	.	.	×	×	.	.	.	×	.	×	.	×	.	.	.	3
4	B.	141	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
58	B.	139	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
57	G.	137	.	.	×	×	.	.	.	×	.	×	.	×	.	.	.	3
65	B.	136	.	.	×	×	.	.	.	×	.	×	.	.	×	×	.	3
109	B.	134	.		.	.	.	.	.	.	.	.	.	.	.	.	.	.
146	B.	133	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	3
100	G.	133	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
106	G.	131	.	.	×	×	.	.	.	×	.	×	.	×	.	.	.	3
26	B.	130	.	.	×	×	.	.	×	.	.	.	.	.	×	×	.	2
119	G.	130	.	.	×	×	.	.	×	.	.	.	.	.	×	.	×	2
3	G.	130	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
39	B.	128	.	.	×	.		.	.	×	.	×	.	.	×	×	.	4
121	G.	126	.	×	.	.	.	.	.	×	×	.	.	.	×	×	.	2
17	G.	126	.	×	.	.	.	.	.	×	×	.	.	.	×	×	.	2
113	B.	124	.	.	×	×	.	.	.	×	.	×	.	×	.	.	.	2
36	B.	124	.	.	×	×	.	.	.	×	.	×	.	.	×	.	×	3
37	B.	122	.	.	×	×	.	.	.	×	?	.	.	.	×	.	×	3
67	B.	121	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	0
	B.	118	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	2
37	B.	117	.	×	.	.	.	.	×	.	.	.	.	.	×	.	×	3
	B.	115	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	4
34	B.	115	.	.	×	×	.	.	.	×	×	.	.	.	×	×	.	2
128	G.	113	.	.	×	×	.	.	.	×	.	×	.	.	×	×	.	3
39	G.	109	.	·×	.	.	.	.	.	×	.	×	.	.	×	.	×	3
45	B.	108	.	.	×	×	.	.	.	×	.	×	.	.	×	.	×	3
156	B.	106	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	3
33	B.	104	.	.	×	×	.	.	.	×	×	.	.	.	×	.	×	3

Table XII. *Parents born in local towns or towns and villages.*

(1) Ref. No.	(2) Sex	(3) I.Q.	(4) Father and mother W.D.C.	D.C.	S.C.	D.T.	S.T.	(5) Father's side (grandparents) W.D.C.	D.C.	S.C.	D.T.	S.T.	(6) Mother's side (grandparents) W.D.C.	D.C.	S.C.	D.T.	S.T.	(7) No. of ancestors born in one village or town
3	B.	150	.	.	×	T.V.	.	.	.	×	.	×	.	×	.	.	.	2
118	B.	142	.	.	×	.	×	.	×	.	.	.	.	×	.	.	.	4
71	G.	133	.	.	×	.	×	.	.	×	.	×	.	.	×	.	×	6
148	B.	130	.	.	×	T.V.	.	.	×	.	.	.	.	.	×	.	×	3
148	B.	125	.	.	×	T.V.	.	.	.	×	.	×	.	.	×	T.V.	.	3
121	G.	120	.	×	.	.	.	.	×	.	.	.	.	×	.	.	.	3
149	G.	120	.	.	×	.	×	.	.	×	T.V.	.	×	.	.	.	.	2
3	B.	107	.	.	×	T.T.	.	.	×	.	.	.	.	.	×	.	×	5
71	B.	106	.		×	T.V.	.	.	.	×	.	×	.	.	×	V.V.	.	3

T. = Town. V. = Village.

Table XIII. *Parents born in distant towns.*

(1)	(2)	(3)	(4)					(5)					(6)					(7
			Father and mother					Father's side (grandparents)					Mother's side (grandparents)					No. ances bo in vill or t
Ref. No.	Sex	I.Q.	W.D.C.	D.C.	S.C.	D.T.	S.T.	W.D.C.	D.C.	S.C.	D.T.	S.T.	W.D.C.	D.C.	S.C.	D.T.	S.T.	
4	B.	140	.	×	.	.	.	.	.	×	V.V.	.	.	×	.	.	.	?
148	B.	138	.	.	×	.	×	.	.	×	.	×	.	.	×	.	×	?
109	B.	134	.	×	.	.	.	.	.	×	.	?	.	.	×		?	?
121	B.	131	.	×	.	.	.	×	.	.	.	.	×	.	.	.	.	?
100	G.	129	×	.	.	.	.	.	×	.	.	.	.	.	×	.	×	?
16	B.	129	×	.	.	.	.	.	.	×	L.	.	.	.	×	.	×	?
141	B.	127	.	.	×	L.	.	.	.	×	L.	.	×	L.	.	G.	.	?
43	B.	127	.	×	.	.	.	.	.	×	V.V.	.	.	.	×	L.	.	?
25	G.	125	×	.	.	.	.	×	.	.	.	.	.	.	×	V.V.	.	?
81	G.	123	×	.	.	.	.	.	.	×	×	.	.	.	×	.	V.V.	?
121	G.	120	×	.	.	.	.	.	×	.	.	.	×	.	.	.	.	?
121	G.	119	.	×	.	.	.	.	.	.	×	.	.	.	×	.	×	?
71	G.	119	×	.	.	.	.	.	×	.	.	.	×	.	.	.	.	?

V. = Village. L. = London. G. = Glasgow.

It became of great interest, therefore, to determine the birthplaces of the parents of a group of children who scored less than 80 I.Q. This information is given in Table XIV, columns (1) and (2), and in columns (3) and (4) the occupation of the fathers are noted.

Table XIV. *Relationship of* I.Q.*'s below* 80 *to birthplaces of parents and to occupations of the fathers.*

	(1)	(2)	(3)	(4)
Ref. No.	No. of I.Q.'s below 80 in 23 schools	No. of cases where father and mother were born in the same village	Father engaged in agriculture	Father engaged in other occupations
141	2	2	2	—
56	3	2	2	Platelayer
109	1	1	—	Bricklayer
20	3	1	1	Carter, shopkeeper
49	2	1	1	Electrician
167	2	—	1	Sweep
100	1	—	1	—
34	1	1	1	—
146	1	—	—	Coachman
17	3	1	3	—
16	3	1	—	Grave digger, bus driver, groom
45	3	1	—	Carpenter, carter, unknown
125	3	1	2	Brickmaker
57	2	1	1	Soldier
47	3	2	1	Bricklayer, warrener
81	4	—	3	Soldier
39	4	1	3	Newsagent
91	4	4	4	—
83	3	1	3	—
33	6	2	2	Labourer, painter, postman, bricklayer
25	2	—	1	Roadman
76	4	1	2	Carrier, labourer
106	3	1	1	Blacksmith, brickmaker
Total	63	25	35	
Percentage		39·6	55·5	

Comparison with a group of children scoring low I.Q.*'s.*

While the parents of children scoring high I.Q.'s were with two exceptions born in different villages, 39·6 per cent. of the fathers and mothers of children scoring low I.Q.'s were born in the same village and, moreover, this percentage is increased to 61·9 if the village radius is extended five miles.

Of the fathers of this group, 55·5 per cent. were engaged in agriculture and of the mothers a very large percentage were employed before marriage in domestic service.

Thus·	Domestic service	55	63
	Factory workers	5	
	Land workers	2	
	Clerk	1	

While no direct evidence is presented here of the prevalence of intermarriage in small rural communities, the data is in accord with the view often expressed by interested observers that intermarriage is one of the causes of deterioration in the offspring.

VII. Discussion.

It has been noted that in every class of the community there is an immense range of ability in the offspring.

In this enquiry the contrast is not between the children of professional men, enjoying every environmental advantage, and the children of labourers with an entirely different upbringing, for we have seen that not more than 16 per cent. of the ancestors of the selected group were persons belonging to the great middle class.

The superiority of approximately 50 per cent. of the selected group is associated with superiority in the ancestry as measured by status.

In the remaining cases, superiority of the offspring cannot be so explained, but given good original stock it does not follow that this trait will be reflected in a rise of status: particularly is this the case in a rural community where without financial resources little improvement in status was possible before 1902 and is still by no means sure. Hence the good stock that has not been affected by intermarriage remains and gives rise to a small percentage of notable children. In this connection we have seen that the children of superior ability under consideration are almost invariably the offspring of parents born in different villages while a significant percentage of mothers and fathers of children of inferior ability were born in one village. It follows that if the birth

of parents in different counties or different villages is a favourable factor for the offspring and the birth of parents in the same village an unfavourable factor we should expect the birthplaces of ancestors of children scoring 104 to 126 (Table XI, second half) to be more static than the birthplaces of ancestors of children scoring 128–153 (Table XI, first half).

The figures are given in Table XV.

Table XV.

		First half of Table XI	Second half of Table XI
	Favourable factors.		
Father and mother	Widely different counties	1	0
	Different counties	1	4
	Different village	11	11
	Total	13	15
Grandparents	Widely different counties	—	—
	Different counties	5	2
	Different village	14	13
	Total	19	15
	Unfavourable factors.		
Father and mother	Same village	2	0
Grandparents	Same village	9	14
	Total	11	14

The differences to be observed are only slightly in favour of the 128–153 group but all the children under consideration are above normal. Had data been available for a complete age group (see Table XIV) the differences would undoubtedly have been marked.

General observations only can be made on the question of environment.

There is little difficulty in detecting children of superior ability soon after they begin attendance at school, so that if superior environmental conditions of any kind are factors of moment the period up to 5 years of age is one of active influence. For a large proportion of the cases considered in Table X superior environment of the kind associated with favourable economic status was not a factor. What, for instance, were the superior environmental conditions in the cases of Index Nos. 65 and 146?

Other environmental factors may, however, be of moment, and of these the following appear to be promising lines of enquiry[1].

[1] Since writing this section an interesting paper by Mary M. Wentworth on "Individual differences in the intelligence of children" has come to my notice. *Harvard Studies in Education*, 1926, VII, 35.

What is the effect of the mother's temperament on the mental development of young children?

What is the effect of bilingualism in home and school?

Is the language of the home—or the absence of it—a factor of supreme importance in the mental development of the offspring during the period before attendance at school begins?

Are initial defects in speech correlated with mental retardation at the age of 6 or at the age of adolescence?

Acknowledgment of the help and co-operation of teachers has been made. To Dr P. H. Ballard for helpful comments in the early stages of this enquiry I offer my thanks.

To Prof. Godfrey H. Thomson I am much indebted not only for the data supplied with reference to a Northumberland Age Group but also for the interest he has taken in the subject of this paper.

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(*Manuscript received* 15 *June*, 1928.)

PUBLICATIONS RECENTLY RECEIVED

An Outline of Social Psychology. By J. R. KANTOR. Chicago: Follett Publishing Co. 1929. Pp. xiv + 420. $2.40.

It is a little difficult to see exactly what this book is intended to be. In scheme it is a comprehensive text book, meant to introduce the student to the whole range of its subject-matter. But the treatment is spread out and lacks conciseness, so that no critical points seem to stand up very clearly. It goes without saying that Prof. Kantor shows a wide knowledge of the work that has preceded his own. Often he gives just that kind of excellently selected list of further readings which can be made only by an author of wide knowledge and good judgment. Also he has tried to think out carefully all that he wishes to say. He gives, too, plenty of illustrations. Yet the impression remains that on the whole the work is on a rather ordinary level of generality. There is a fondness for heavy terminology.

The position is that social psychology is a science of cultural conduct, and cultural conduct is (*a*) stored by a set of people, and (*b*) determined by a set of people. Prof. Kantor reviews the various ways in which such conduct has been or may be viewed; discusses its expressions and their functions; considers the bearing of these expressions upon individual conduct, and how they enter into typical 'humanistic situations,' such as 'the scientific situation,' 'the political situation,' and so on. It all seems excellent as a scheme, and yet it all remains remote and somewhat blurred n the book. Perhaps he has tried to cover too much ground.

The History of Psychology. By W. B. PILLSBURY. New York: W. W. Norton Co. 1929. Pp. x + 326. $3.50.

On the whole this is a very good book for the student to read. It is rather of the nature of a catalogue of names with notes attached to each name. But the selection of names is good and so far as the notes are concerned with an exposition of doctrines they are as accurate as they could well be within the limits of space imposed. Prof. Pillsbury casts a very wide net, for he begins with *Early and Classical Greek* psychology and ends with McDougall, the *Gestalt* psychologists, and the school of Spranger, Binswanger and Ewald. Sometimes he is astonishingly inaccurate as to his facts. The worst example of this is probably the brief reference to Dr W. H. R. Rivers, who is said to have been "connected with St John's College, Cambridge, in various ranks until he became its head." It would have been extremely easy to find out what Rivers's position at St John's College actually was. However, criticism of this kind of book is undoubtedly extremely easy. Taking it all through, it is an interesting and useful volume.

Luc de Clapiers, Marquis de Vauvenargues. By MAY WALLAS. Cambridge University Press. 1928. Pp. viii + 308. 12*s.* 6*d.* net.

The first 150 pages of this book deal with Vauvenargues's life. The story is exceedingly well told and is uniformly interesting, though it is not exciting. The next 70 pages or so deal, mainly in a descriptive but to some extent in a critical manner, with those writings of Vauvenargues which have some psychological bearing. Chief of these was his championship of intuition and feeling as being of very great importance in setting the course and content of thinking. Herein, the author maintains, Vauvenargues was well ahead of his time. The final portions of the book are concerned chiefly with Vauvenargues's ethical views. In the end Miss Wallas sums

up and maintains that her hero's "notes on the psychology of thinking have an especial interest in the present time," and must be of permanent value because they are based upon direct, "sensitive and vivid observation."

For the volume as a whole there can be nothing but praise: it is a sympathetic, attractive and accurate piece of work. That it is a little spun out, and that Vauvenargues still remains a distinctly minor character in the history of psychological development, are not the faults of the author.

Scientific Method: its Function in Research and in Education. By TRUMAN L. KELLEY. Columbus: The Ohio State University Press. 1929. Pp. 195. $2.50.

The book reports five rather slightly connected lectures given by Dr Kelley at the State University of Ohio. The lecture form is adhered to throughout. Consequently here and there the book seems unnecessarily diffuse, and the style is somewhat conversational. Nevertheless Dr Kelley has many interesting and important things to say, and his study is well worth the time it takes to read. The first lecture deals with broad differences between different fields of research and the bearing of these upon the methods of investigation that must be adopted. The second lecture, called The Role of Judgment in 'Objective Measurement,' is largely a consideration of the vices and virtues of the questionnaire, and on the whole concludes it to have more of the latter than of the former. The third lecture asks In what Units shall we measure Intelligence and Achievement? and the fourth follows this up by considering the significance of recent scientific development upon research in education and heredity. The last lecture is somewhat apart from the others and consists of a well illustrated study of the mental characteristics of men of science. Dr Kelley makes a valorous and interesting attempt to say what mental characteristics a great man of science must possess, and adds a further list of traits which he very often does possess. It is a book that presents much material for discussion and might well be a starting point for many important investigations.

Studies in the History of Statistical Method. By HELEN M. WALKER. Baltimore: The Williams and Wilkins Co. 1929. Pp. viii + 229.

The topics dealt with are: The Normal Curve, Moments, Percentiles, Correlation, The Theory of Two Factors, Statistics as a Subject of Instruction in American Universities. A very useful glossary of terms is added under the title: The Origin of Certain Technical Terms used in Statistics. An appendix deals with The Study of College Catalogues, and there is a capital bibliography. Several excellent portrait illustrations adorn the book. It will be seen that the book is rather a collection of studies of selected topics than a systematic history. Sometimes even there, as in The Theory of Two Factors, the treatment is no more than an annotated bibliography. As a most useful book of reference, however, the volume deserves to be widely known.

The Pedagogical Value of the True-False Examination. By A. W. COCKS. University Research Monographs, No. 7. Baltimore: Warwick and York. 1929. Pp. x + 115. $2.60.

As a result of his interesting experimental survey Mr Cocks concludes that the 'true-false test' is definitely superior to the essay type of examination. He gives his reason for this in detail, and discusses, always in the light of actual results, various means of making the test yield its most reliable and fruitful information. The monograph is of considerable interest and is very clearly written.

The Value of Homogeneous Grouping. By T. L. Purdom. University Research Monographs, No. 1. Baltimore: Warwick and York. 1929. Pp. 99. $2.08.

The author carried out a comprehensive survey of the value of homogeneous grouping in five schools in Missouri, U.S.A. and here records his results in detail. On the whole the results do not show any marked advantages of the method of grouping in question. The work appears to have been carried out with care and thoroughness.

Individual Psychological Treatment. By E. Wexberg, trans. by A. Eiloart. London: The C. W. Daniel Co. 1929. Pp. 161. 6*s.* net.

The book gives a clear and readable account of the practical principles underlying the therapeutic work of Dr Alfred Adler. It is one of a series entitled *Psychic Methods of Cure*, and it is greatly to be hoped that the unprofessional reader who takes up one of these books will proceed to read them all. If he does not he may be led to attempt immediate application on the basis of very one-sided and inadequate knowledge. The danger of this is apparent. And when, as undoubtedly in the present case, the exposition is particularly clear and simple, it is a danger the results of which may be extremely serious. Subject to this caution, the book may safely be recommended as a good presentation of its subject-matter.

Problems in Psychopathology. By T. W. Mitchell, M.D. London: Kegan Paul. 1927. Pp. v + 190. 9*s.* net.

This volume contains eight lectures given to members of the British Institute of Philosophical Studies. Beginning with an account of the 'pre-Freudian' school of Janet and Morton Prince, Dr Mitchell shows how the former's descriptive account of dissociation, as being due to some lack of psychic tension, is inadequate in that the mechanical effects of decrease of tension are given, but no explanations are offered of the dynamic causes of the decrease or of the reasons why hysteria may develop in one case and psychasthenia in another. These explanations were given by Freud, who introduced the concepts of Libido, repression and the unconscious. An account of his work takes up practically the whole of the rest of the book. The gradual development and elaboration of Freud's theories are traced from their beginnings in the treatment of hysteria, while the present day psycho-analytical views and problems are presented with a clarity and with a restraint which is not usual. The result is a volume which can be confidently recommended as an introduction to the large mass of psycho-analytical literature.

The 'post-Freudians' (as Dr Mitchell calls them) Rivers, Jung and Adler, receive scant attention; but, in the limits prescribed by the lectures, this was unavoidable.

Clinical and Abnormal Psychology. A Text book for Educators, Psychologists and Mental Hygiene Workers. By J. E. Wallis Wallin, Ph.D. Boston, New York, Chicago, Dallas, San Francisco: Houghton Mifflin Co. 1927. Pp. xxii + 650.

This volume has been specially prepared for those whose duties bring them into contact with educational clinics where individual children are examined. As such it deals primarily with the various forms of tests—intelligence, sensory, memory, motor ability, temperament etc.—and with the methods of carrying out a psychological examination, the difficulties of which are dealt with at some length. The whole is connected together by an outline of normal and of abnormal psychology.

The work is to be recommended. It is well balanced and the various views and theories are put forward and criticized with lucidity and restraint. Its value is considerably increased by the classified bibliography of over fourteen pages of titles.

Die Hauptrichtungen der gegenwärtigen Psychologie. Von R. MÜLLER-FREIENFELS. Leipzig: Quelle und Meyer. 1929. 145 S. M. 1.80.

This book should be useful to the student who, having already attained a fairly stable point of view, wishes to see whether or not there is any order in the apparent chaos of current psychological theories. After a general introduction, Prof. R. Müller-Freienfels divides current theories into two groups: those having a general objective trend, and those having a general subjective trend. In the first group are: association psychology, Wundtian psychology, theories having a general sensory motor basis, the views of E. Jaensch, the work of the Würtzburg school, Gestaltpsychologie, Behaviourism, and the study of conditioned reflexes, and a rather indefinite range of views called 'Parallelisten und Dualisten.' In the second group are: phenomenology, personality theories, 'Lebenspsychologie' (Avenarius, Groos, R. Müller-Freienfels), psychoanalysis, individual psychology (Adler), medical psychology (Binswanger, Kritchmer, etc.), characterology, and 'Die geisteswissenschaftliche Psychologie' (Dilthey, Spranger). All these are briefly characterized. References to appropriate literature are given, and in some cases critical studies are also indicated. Whether, having perused all this, the student will find his views clarified is more than doubtful: but at least he will have plenty to think about.

Grundfragen der Wahrnehmungslehre. Von P. F. LINKE. München: Ernst Reinhardt. 1929. xxvi + 430 S. M. 13.16.

This able and critical survey of general theories of perception is substantially a reprint of the first edition, but includes about fifty pages of 'concluding remarks.' This addition is concerned at length with developments of Husserl's phenomenology, rather briefly with the work of Joseph Geyser and some views of Driesch, and with Stumpf's *Empfindung und Vorstellung*, and again at length with recent contributions of *Gestalttheorie*. All these are considered in their general theoretical rather than in their experimental aspects. The volume remains one worthy of the most careful study of all students who are attracted by the epistemological aspects of perceptual problems.

Psychologie der frühen Kindheit. Von W. STEEN. Leipzig: Quelle und Meyer. 1928. xv + 539 S. M. 15.80.

Prof. Steen's book is already well known to English readers from its translation. It has now reached a fifth edition and has been thoroughly revised, and is expanded in various parts. It contains also a section by Prof. Kurt Lewin dealing with the psychological significance of the movements of the young child. This is illustrated by reproductions from Prof. Lewin's cinematographic records. A further brief section by Prof. Heintz Werner deals with 'magical' reactions in the young child.

The book is a classic of observational and experimental studies of the more qualitative kind, and can be neglected by no student of the psychology of early childhood.

Freud's Tragischer Komplex: Eine Analyse der Psychoanalyse. Von CHARLES E. MAYLAN. München: Ernst Reinhardt. 1929. 215 S. M. 7.80.

The author cheerfully and at length analyses psychoanalysis. He builds upon early recollections and dreams of Freud, and finds that much can be explained in the light of Jewish tradition and belief. The writing is lively enough, but most readers may think the explanation somewhat forced. However it may be admitted that the author has succeeded in an attempt to provide an amusing entertainment, and a great many of his remarks are undoubtedly very shrewd.

Psychologie. Von H. K. Schjelderup. Berlin und Leipzig: W. de Gruyter. 1928. xii + 330 S. M. 10.

This German translation of Prof. Schjelderup is the work of Max von Leinner. The text book itself follows traditional lines in the main. After a general introduction dealing with methods and historical development, the author states his general point of view. This is on the whole very much in line with modern functional and genetic psychology. The book then deals in turn with sensory responses, instinctive tendencies, temperament and intelligence, with the general principles of apprehension and learning, with memory reactions in most of their many forms, with personality and difficulties arising from its development. A short final section deals with modern applications of psychology. The treatment is judicious as well as comprehensive and shows a wide acquaintance with the best psychological work of several countries. As an introduction in general terms the volume is undoubtedly a good one.

Kindheit und Jugend: Genese des Bewusstseins. Von Charlotte Bühler. Leipzig. 1928. xx + 307 S. R.M. 10– .

This is the third volume of the series of psychological monographs edited by Prof. Karl Bühler the general purpose of which is to build up the psychological history of the human individual from his birth onwards. It is a thoroughly interesting and workmanlike production, containing not a mere survey of the contributions of other writers but the record of a mass of important observational and experimental research carried out by the author and her assistants. The story proceeds from the first year to the period of adolescence. Successive chapters deal with the characteristic reactions of the first year, of years two to four, of years five to eight, of years nine to thirteen, and of years fourteen to nineteen. Naturally the most detailed studies are of the earliest years, but throughout the treatment is marked by a wide range, considerable sympathetic insight, and extremely clear exposition.

Contributi del Laboratorio di Psicologia e Biologia. Serie Terza: Pubblicazioni della Università Cattolica del Sacro Cuore. Milano. 1928. Pp. 436. L. 40.

This volume contains some unusually interesting records of research. First comes a long paper by Necchi Ludovico on *Ricerche medico-statistiche sui fanciulli anormali.* This is followed by two papers by Pastori Giuseppina dealing with medical and psychological problems connected with the functioning of the pineal gland. Castiglioni Giulio contributes a well documented study of the idea of God as he has found it in girls. An experimental study of eidetic imagery by G. M. Vacino, illustrated by a number of actual reproductions, follows. Gemelli Agostino contributes two papers, one a general introduction to the study of perceptual process, and the second an experimental investigation of the appearance and disappearance of perceived forms. Two studies by Gatti Alessandro are also included: an illusion in the field of kinaesthetic-tactual experience, and perception of wholes through the successive presentation of parts. Finally Larna Alberto writes on the tactual appreciation of form. All these papers are well presented, the whole work deals with genuine problems, and wherever it is experimental the methods are admirably adapted to the questions studied.

A Sceptical Examination of Contemporary British Philosophy. By Adrian Coates. London: Brentano's Ltd. 1929. Pp. 256. 10*s.*

A psychologist with a strong taste for general theory would find this book attractive. In ten short chapters ten contemporary philosophers are attacked or supported—mostly attacked. Mr Coates writes with energy and a refreshing lack of reserve.

Types of Philosophy. By W. E. HOCKING. London: Charles Scribner's Sons. 1929. Pp. xi + 462. 7*s.* 6*d.* net.

Prof. Hocking having very lucidly discussed a large variety of views put forward by different philosophers, at length confesses that he is a kind of idealist, and briefly explains what kind.

A Modern Theory of Ethics. By W. OLAF STAPLEDON. London: Methuen and Co. 1929. Pp. ix + 277. 8*s.* 6*d.* net.

The sub-title of this book is: A Study of the Relations of Ethics and Psychology. Dr Stapledon considers that we mean by 'ethically good' that which "is the fulfilment, or progressive fulfilling, of teleological tendencies objective to consciousness." This obviously lands him in an attempt to say definitely what the notion of tendency in modern psychological discussion really involves. His remarks under this head and in the ensuing discussion of 'psychical conflict' are well worth study, whether or not his application of them to ethical problems is accepted. Naturally they still leave a number of difficulties, as, for example, whether tendencies are in any sense structural. The attempt to clear up the expression "teleological tendencies objective to consciousness" is valiant, but unsatisfying.

SEVENTH INTERNATIONAL CONGRESS OF PHILOSOPHY.

(*First Circular.*)

In accordance with the arrangement made by the Permanent International Committee in 1926, the Seventh International Congress of Philosophy will meet in September 1930 at Oxford.

The date of the Congress will be September 1st to 6th, 1930.

The recognized languages will be English, French, German and Italian.

The programme of the Congress will follow the lines of the Programme for the Sixth International Congress held in America in 1926. The Sessions will be arranged in four Divisions: A. Metaphysics; B. Logic and Epistemology; C. Ethics and Political Philosophy; D. History of Philosophy. Each Division will have one general meeting, and each Section a special meeting. The papers will be read by specially invited delegates from the various countries. The number of speakers to be fixed beforehand cannot exceed for a Divisional general meeting 4 or for a Sectional special meeting 3. The time allotted to papers will be 20 minutes each.

Membership in the Congress will include Active Members and Associate Members (the families of Active Members). The fee for Active Members will be £1 and for Associate Members 10*s*. Active Members will be entitled to a copy of the *Proceedings*.

All correspondence regarding the Congress should be addressed to Mr A. H. Hannay, Secretary and Treasurer, 40 Well Walk, Hampstead, London, N.W. 3.

The detailed plans for the Congress and arrangements for accommodation will be announced later.

For the Organizing Committee.

J. A. Smith, *Chairman.*

A. H. Hannay, *Secretary.*

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

SECTIONAL MEETING.

MEDICAL.

June 26th, 1929. "An Analysis of Psycho-sexual Divalence in Women." By Dr E. H. CONNEL.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AT 30 SEPTEMBER 1929

Revenue Account for the year ended 30 *September* 1929

Expenditure

Year to 30 Sept. 1928 £ s. d.	Expenditure	£ s. d.	£ s. d.
50 0 0	*To* Rent		50 0 0
59 6 6	Printing		79 6 6
24 14 1	Hire of Halls		37 15 9
	Office and General Expenses:		
	Secretarial Fees	200 0 0	
	Addressing	17 10 3	
	Stationery	14 10 8	
	Postage and Telephone...	38 11 0	
	Audit Fee	15 15 0	
	General Expenses ...	13 18 2	
299 16 4			300 5 1
435 1 8	Journals Publication Expenses		262 2 2
15 6 1	Teas		11 3 9
5 2 8	Scottish Branch Expenses		8 0 0
6 0 0	Midland Branch Expenses		2 12 6
4 3 0	Bank Charges		10 0
23 19 5	Committee for Research in Education ...		— — —
48 12 3	Library Depreciation		48 13 11
972 2 0			800 9 8
242 11 0	Balance, being Surplus of Income over Expenditure		337 15 2
£1214 13 0			£1138 4 10

Income

Income	£ s. d.	£ s. d.	Year to 30 Sept. 1928 £ s. d.
By Subscriptions 1929	1210 3 0		
1928	7 17 0		
Arrears written off subsequently received, etc. ...	30 12 6		
	1248 12 6		
Less, Resignations and Removals	201 12 0		
		1047 0 6	1126 5 0
Midland Branch (3 Associate Members) ...		7 6	— — —
Interest on Deposit Account		2 8 10	— — —
Interest on Investments		88 8 0	88 8 0
		£1138 4 10	£1214 13 0

	Rent	12 10 0	
	Scottish Branch	8 0 0	
	Midland Branch	2 5 0	
12 10 0			22 15 0
60 14 0	*Subscriptions in Advance*		50 14 6
	Publications Reserve		
172 0 0	Balance as at 1 Oct. 1928		172 0 0
	Revenue Account		
	Balance at 1 Oct. 1928 ...	3086 4 11	
	Add, Surplus of year ...	337 15 2	
3086 4 11			3424 0 1
£3331 8 11			£3669 9 7

Balance at Barclays Bank			
Deposit Account	500 0 0		
Less,			
Current Account Overdrawn	64 14 11		
		435 5 1	79 10 10
Investments (at cost)			
£100 New South Wales $5\frac{1}{2}$ % Loan 1924–34	100 0 0		
£1000 4 % War Loan 1929–42	1013 5 6		
£1100 5 % War Loan 1929–47	1117 6 9		
	2230 12 3		
Note. Market value of above securities at 30 Sept. 1929 £2207. 15*s.* 0*d.*			
500 National Savings Certs. (dated 20 Feb. 1923) ...	400 0 0		
		2630 12 3	2630 12 3
Subscriptions in arrear		165 6 6	183 15 6
Library			
Balance at 1 Oct. 1928 ...	437 10 4		
Expenditure during year ...	49 9 4		
	486 19 8		
Less, Depreciation	48 13 11		
		438 5 9	437 10 4
(Of the Expenditure on Library of £150 authorised on 8 March 1928 the sum of £110. 17*s.* 9*d.* has been spent to date leaving a balance of £39. 2*s.* 3*d.* unexpended.)			
		£3669 9 7	£3331 8 11

Note. Stock of Journals. There exists a number of past and present volumes of Journals in Stock, but owing to the possibility that a large proportion of these may never be sold, no valuation can be placed on such stock.

We beg to report that we have examined the foregoing accounts with the books and vouchers of the British Psychological Society and that we have obtained all the information and explanations we have required. In our opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of our information and the explanations given to us and as shown by the books of the Society. Investments, which are lodged with the Bank, have been verified by production of Banker's Certificate.

35, Copthall Avenue, London, E.C. 2.

21 *November* 1929.

A. J. HARPER & CO.
Incorporated Accountants.

COMMITTEE FOR RESEARCH IN EDUCATION

ANNUAL REPORT.

The Committee for Research in Education has met twice during the year 1928–9. It has been mainly concerned with the formulation of problems on which research in education is urgently needed and in collecting information with regard to the need of financial help for work that is in progress. Professor Burt and Mr Tibbey very kindly undertook to prepare a memorandum on the subject.

The following problems have been stressed as some on which research is specially needed:

1. The improvement and standardization of psychological and educational tests, and including not only tests of intelligence and of the ordinary Elementary curriculum, but also of special psychological capacities such as memory in its various forms, etc.

2. The validity of the Junior County Scholarship Examination as a means of selecting abler children; the investigation of supplementary methods, *e.g.* intelligence tests, teachers' judgments, etc.

3. The reliability and standardization of teachers' judgments generally, including judgments of temperamental and moral qualities as well as of intellectual and educational.

4. The characteristics of the supernormal child, family history, home circumstances, development, special capacities, temperament and character.

5. The handicaps of the child from the poor home.

6. The effect of absence from school on school attainments (it has been suggested that many children who do not begin reading or calculating until comparatively late in their school life nevertheless after a few years usually reach the attainments proper to their mental age. How far is this true?).

7. The causes and treatment of special disabilities in reading, spelling, and arithmetic.

8. Collating methods of treating dull and backward children.

9. The psychology of the partly deaf and the partly blind, with special reference to their education.

10. The effect on school progress of minor disabilities such as left-handedness, stammering, etc.

11. The relative value of special methods of teaching particular aspects of elementary subjects: *e.g.* in reading, the look-and-say method, the phonic method, etc., etc.; in arithmetic, the various methods of teaching subtraction, proportion, etc.

12. The nature, frequency, and treatment of neurotic children.

13. The change in character-qualities at 11. (The clean-cut implies that after the age of 11 children require a different outlook, and that this need is dependent on chronological or physiological age rather than upon mental age. This is an assumption that needs to be checked. One approach might be to examine the different character-qualities of individuals having the mental age of 11, whether those individuals were borderline defectives of 15 or 16 or supernormal children of 8 or 9.)

It has been found that work on the following topics is in progress and that the workers would welcome financial support for the continuance of the work.

1. Mental Characteristics of Physically Defective Children.
2. Young Children's Language.
3. Plateaux in Learning.
4. The Effect of Incentives on Learning.
5. Modern Studies in Temperament and their Educational Significance.
6. Educational Problems connected with Bilingualism.

Inquiries were sent to the training colleges with regard to the syllabuses drafted by a joint committee of some members of this committee and of the Training College Association. The replies showed that the syllabus is regarded as too difficult and too comprehensive for use by lecturers in two-year elementary training colleges.

Twelve inquiries with reference to tests and methods have been answered, and the summary of research in progress appeared in the April number of the *Journal*.

V. HAZLITT.
Hon. Secretary.

FINANCIAL STATEMENT FOR YEAR ENDING, 31ST OCTOBER 1929.

RECEIPTS.

	£	s.	d.
By Balance	14	10	8
,, Special receipt		6	2
	£14	16	10

EXPENDITURE.

	£	s.	d.
To Typing and duplicating	3	1	9
,, Postages	2	13	6
,, Printing	1	0	0
,, Hire of Rooms	1	0	0
,, Balance	7	1	7
	£14	16	10

Audited and found correct.
T. G. TIBBEY.
26 *November* 1929.

Signed, A. E. TWENTYMAN,
Treasurer.